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CONTENTS


| | Page |
|---|---|
| Report of Investigations into an Obscure Function of the Subconscious Mind. Hugh Woodworth | 185 |
| A Summer Session Course in Psychical Research at Harvard University. Gardner Murphy | 231 |
| Picture-Tests. Rev. W. S. Irving | 236 |
| Index to Volume XXXVI (1942) | 262 |


## Report of Investigations into an Obscure Function of the Subconscious Mind*


HUGH WOODWORTH


[We regard this paper as a highly suggestive contribution to the analysis of certain problems in parapsychology, and it is published for that reason. The data are not treated by the currently accepted statistical procedures; on the basis of our own computations, based on the data given, we can not agree with the author's view that they possess statistical significance. The method is exploratory and undoubtedly merits further development.—ED.]

As the present report† deals with experiment and theory in a new field, a few preliminary remarks may help to orient the reader.

1. *The Factual Material*—All experiments referred to were performed with the ESP cards used at Duke University. Results of the experiments are hence numerical, and lend themselves to mathematical analysis and appraisal.

2. *The Present Thesis*—Although other investigators, notably Dr. Rhine, have used the cards chiefly to demon-


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†A less complete edition of the report was published privately in July, 1941.

strate the existence and to explore the nature of clairvoyance and telepathy (collectively referred to as extra-sensory perception or ESP), my own investigations are not aimed at either of these faculties as such, but rather at an obscure subconscious mechanism of which clairvoyance, at least, proves to be a variation.

3. *Plan of Presentation*—It has seemed most natural to present the experiments and reasoning in the order in which they occurred: certain of Rhine's earlier findings are mentioned first; then the speculations and the hypothesis they gave rise to; finally are presented the experiments by which the hypothesis was tested.

As will be seen, the central or theoretical section of this sequence preponderates, yet I think its importance will justify its length. Presentation of the experiments may seem delayed, and perhaps subordinated; yet if they appeared earlier their meaning and significance would be less apparent.

4. *Appraisal of Results*—One of the most knotty points in ESP work has been the question whether a set of figures purporting to demonstrate ESP is in reality indicative of anything other than the operations of pure chance. To answer the question, many branches of mathematics have been invoked, and it has been generally decided that before results can be considered truly evidential of ESP they must deviate from chance expectation by a predetermined (and, it should be remembered, an arbitrarily determined) "mathematically significant" extent.

This is a highly desirable requirement, yet the emphasis it has received has had the effect, I think, of blinding some investigators to facts which, although not mathematically significant (according to the set standard), nevertheless may constitute evidence of value. The point that seems worth stressing is that results of ESP investigations might well be subjected to two entirely different forms of scrutiny. There are, after all, two problems and two answers, and it does not follow that a test for the first answer will conveniently fit the second.

The first problem is, *Does ESP exist?* and if evidence is offered that it does, then we are justified in demanding that such evidence be definitely outstanding, and beyond any reasonable dependence upon chance.

The second problem may be stated, *What are the characteristics of ESP?* But in setting out to solve it we must first decide whether the previous question has been answered affirmatively or not. If it has not been, and the reality of ESP itself remains in doubt, then all evidence seeming to throw light upon its characteristics should measure up to strict requirements for mathematical significance. But, on the other hand, if it has been definitely shown that ESP exists, we are then justified in regarding all experimental results with new interest and new credence. We must look now, not for overwhelming weight of evidence, but for clues. The original standards, set deliberately high, are now too rigorous, and if employed might compel us to dismiss the very material in which the most important clues may lie.

By its very nature, of course, a clue must stand out somehow from its matrix, and at first glance it may seem that this characteristicness would be mathematically significant, or could be made so with a sufficient volume of experiments. In some cases this is true, and the clue evidence meets present standards for significance. Frequently, however, a clue's characteristicness is so small that the volume of experiments necessary to establish it unequivocally is impractical to perform, and in still other cases the clue is variable, or after a time disappears, with the result that in a volume of experiments it would cancel out, or tend to water down to zero.

It is hence suggested that a clue to ESP characteristics, to be considered such, provisionally at least, should be allowed to rest on a visible consistency over a reasonable length of time—unsatisfactory as this requirement may appear. Recognition of clues, after all, is properly a matter for sheer experience and judgment, as witness the value

attached to the opinions of the best doctors, detectives, psychiatrists, geologists, etc. Wherever evidence is subtle, arbitrary yardsticks tend to defeat their ends.

The two attitudes possible toward ESP evidence are well illustrated in a man who is asked if his room has been burglarized. He looks around, finds nothing glaringly amiss, and answers, No. But let him learn that a stranger has been seen leaving his window by a ladder. His scrutiny of the room is now of a different order; a misplaced ornament, a drawer half-open, a scrap of mud on the rug—all appear in a new light, and he is entirely justified in suspecting their significance. The evidence that of itself would not prove the burglar's visit can nevertheless be of value to show where he has been.

In the present paper some of the evidence offered is sufficiently outstanding to satisfy high standards of significance, and some is not. It is to be hoped that the latter will hold at least enough interest to justify its presentation here.

Before proceeding with the main thesis it may be well to describe briefly the method of ESP investigation used by Rhine and others, and repeatedly referred to in this report.

To test people for clairvoyance, telepathy, and one or two other extra-sensory faculties, a special deck of cards is used, and the "subject" is asked to make a series of guesses as to what the concealed faces of the cards may be. As due precautions are taken to prevent the subject learning the identity of the cards by any known means, it is assumed that any successes beyond those that can be attributed to chance indicate that he has obtained his information in some extra-sensory way; for example, by telepathy or clairvoyance.

The ESP deck consists of twenty-five cards, each bearing one of five symbols: cross, star, circle, square, and wavy lines. Thus the pack contains five crosses, five stars, five circles, etc. In handling the cards a number of methods

have been developed and made use of, some of which will be described later. The key fact is that since there are five different symbols, anyone attempting to guess them can be expected to guess right once in five times, on the average. This is the "probability" or "chance expectation" and is mathematically determinable.

Of course, when the number of cards guessed is small, wide variations from this *average* expectation will normally be encountered. For instance, in 25 "trials" (a single run through the pack), although the *probability* is 5 hits, it is to be expected that a subject will frequently guess right as many as 8 or 9 cards—or as few as 1 or 2. But as the number of cards guessed is increased, the laws of chance demand that the average score approach more and more closely to the expected average score of 5. To illustrate: in a single run of 25 cards it is less unusual (on chance grounds) to obtain a score of 11 than it is to obtain, in 100 runs, an *average* score of 6. It follows that a score must always be considered in conjunction with the number of guesses that determined it.

Whatever the score, it can be evaluated for ESP; the odds against a chance explanation are calculated, and when these are long enough the score is labelled "significant"—indicative, that is, of some factor other than chance in the guesswork, some way of knowing that must be considered extra-sensory.

My acquaintance with modern ESP research dates from 1937, when a copy of Rhine's *Extra-Sensory Perception* (Faber and Faber, 1935) came to my attention.* In this book, containing the records of those people whose card guesswork produced different-from-chance results, it struck me as remarkable that although in the majority of such cases subjects guessed right more often than chance would account for, it occasionally happened that someone would guess right *consistently less often* than could be expected on

*The first edition of *Extra-Sensory Perception* was published in 1934 by the Boston Society for Psychic Research. The page numbers given here refer to the Faber and Faber edition.—Ed.

purely chance grounds. On the one hand were scores averaging 8, 9, or more over hundreds of trials; and on the other, there were rare instances where guesswork produced scores averaging significantly *below* 5.

Commenting on some experiments similar to his own made by Dr. Estabrooks, Rhine writes on page 27 of the work mentioned above, ". . . one group ran considerably below chance expectation when asked to run a second series. . . ."

On page 191 he summarizes the work of Mr. Harvey L. Frick which "consists of 900 trials, run 100 per day at a single sitting. The scores are totalled for the ordinal 20's, making five subdivisions of the total, each representing the 20's in the run of 100. . . . Frick's scoring fell off by steps of 20's and actually went below chance expectation on the last two 20's." The deviations (p. 74) were: plus 13, plus 5, plus 3, minus 7, minus 9.

Rhine also refers (p. 193) to "Linzmayer's drop below chance to 4.5 in 25 for 600 trials in June, 1931, immediately following 600 trials at more than twice that rate, 9.9." Again, on page 175 in a table summarizing the effect of illness on extra-sensory perception, Linzmayer's average during a cold is shown to be 4.3 per 25 over 350 trials.* Rhine says (page 86), "While not perhaps finally proved, there appears pretty clearly to be some definite reversal of the function, a kind of negativistic clairvoyance."

From the time I first read *Extra-Sensory Perception* this occasional below-chance scoring struck me as particularly significant. In the first place, the phenomenon itself is arresting, for how is it possible for a man to guess *less* successfully than the laws of chance would demand? How can he make more errors in his guesswork than a machine, such as a roulette wheel, would produce? If he allowed the fall of a coin to determine every call, his percentage of correct guesses would inevitably go *up*.

*This low scoring of Linzmayer's, in itself, is scarcely significant. It is only in relation to his usual results that it appears interesting.

And secondly, granting that below-chance scoring does occur, we are immediately confronted with an unmistakable clue to a *mechanism* at work in extra-sensory functioning. A subject reveals by turn unaccountable knowledge, and even more unaccountable "ignorance." Here, then, is not simple waxing and waning of a little-known power, but actual alternation across a zero point. As long as ESP remains, as is said, "positive," we have merely a new faculty, an unsuspected sense revealed. But when this faculty diminishes to zero functioning and then, as is said, "reverses" itself, we have something that is infinitely more substantial and far more complex to investigate.

And finally, most compelling of all questions—in the light of such an alternating faculty, what can we learn about the norm, the normal mind? If the mind is characterized by knowledge, and if a strange factor can bring both knowledge and falseness by turn, may not the norm itself, the normal mind itself, be somehow the *product* of this factor's functioning?

Such are the questions that obtrude themselves; but before attempting to investigate them, one or two preliminary points must be considered.

Below-chance scoring is commonly referred to by Rhine and others as "negative" clairvoyance or a "reversal" of clairvoyance. People who use the terms may not be deceived by them, but as they are essentially misleading I shall mention their inherent fallacy and then discard them.

The concept of clairvoyance, though strange, is one that the mind can grasp. A clairvoyant person "sees"—although without the use of any of his ordinary senses. The faculty can easily be imagined as a new or hidden sense, and high scoring is evidence of its existence.

But to imagine a reversal of the function that would explain below-chance scoring is impossible, for it can have no reversal. Consider a parallel. In the field of ordinary vision there is seeing, and when the eyes are closed, non-

seeing. But to envisage a reversal of seeing quite defeats the imagination. It would mean seeing less than nothing—or at least a positive opposite of "something." To compare ordinary sight with a thermometer, zero would be blindness; degrees above zero, degrees of sight. But what is there to correspond to degrees below zero? It follows that negative clairvoyance, strictly speaking, is a term without meaning, and one that should therefore be avoided.

To some readers the importance I have just attached to below-chance scoring may seem unjustified because of an explanatory hypothesis already in existence. Rhine reports (page 108) how Pearce and Stuart, two of his best subjects, made the experiment of deliberately trying to get low scores. In this they were successful, Pearce showing that he could "alternate high and low scores on request, actually giving on one occasion 9 in 25 when asked for a high-score run, followed by 1 in 25 for low; then a request for high brought 10, and one for low another 1 in 25."

Stuart called 1300 cards, but called each twice, trying first for a hit, then for an error. His low-trying gave him a total of 182 hits against the expected 260; and his high-trying yielded 341. (From table, p. 96). In obtaining their low scores, both men apparently rejected all their first impressions, and as a number of these were presumably correct by ESP, the same number of substituted calls would hence be wrong, and result in below-chance scoring.

Such experiments as these have led some investigators to assume that when low scoring occurs unintentionally the subject is in a "negative" frame of mind that results in his *subconsciously* twisting the results in much the same way that Pearce and Stuart deliberately twisted theirs.

As far as this theory goes it is all right, but the significance of the whole matter is unwittingly skipped over when it is assumed that the action of the subconscious in unintentional below-chance scoring is a *simple parallel* to the conscious steps made by Pearce and Stuart. It no doubt parallels the conscious action to some degree, but important

differences exist that repay consideration. After all, if the subconscious can receive a correct impression, reject it, and substitute another, the matter is worth investigating, and I shall now devote some space to what apparently goes on.

To begin with, as "negative clairvoyance" is obviously a self-contradictory and unsatisfactory expression, I propose the two terms "rejection" and "blocking" to represent whatever the extraordinary action is that causes a subject to produce in his guesswork fewer hits than would be expected to occur if chance alone were operative. "Rejection" can be used when a volitional action needs to be implied; "blocking," when the action is spoken of as a more impersonal or mechanical effect. But in order to study the phenomenon profitably it is necessary first of all to obtain some sort of diagram representing the difference between pure guesswork, and guesswork in which extra-sensory perception plays a part.

Consider a subject who has been shown the five symbols, who has memorized them, and who has been asked to call off a number of them at random, in any order he pleases. No deck of ESP cards is concerned at all; there is no question of guessing anything whatever.

Without hesitation the subject may call, "square, circle, lines, square, lines, cross, circle, star, etc." He could probably give no reason why, in each call, one symbol was chosen rather than another, yet it can be assumed that there was a reason, or reasons (that is to say, a stimulus, or stimuli). The mechanism involved can be investigated better if the process of calling the symbols is slowed down somewhat, so that as long as five seconds elapses between calls. If this is done, the subject begins to realize that each call is apparently determined by idea associations, sound associations, kinesthetic associations (the most natural sequence of lip and tongue movements, etc.), associations of visual imagery, natural preferences, relative dominance of certain symbols, etc.

(The slowing down of the calling process probably increases the number and variety of associative trains that can be operative, i.e., fast calling may depend on sound and kinesthetic associations only; but this consideration falls outside the present study. My object in introducing the time element is simply to reveal detail, as in a slow-motion film, to obtain an idea of what probably goes on more rapidly and less consciously in faster calling. I assume that the mechanics of slow and fast calling are *approximately* similar.)

To express the situation diagramatically, then, it is obviously necessary to show two things: (1) A center of choice, which would appear to be the central ego, the self-surveying and volitional "I," and (2), the five symbol images or symbol concepts. (Neither "image" nor "concept" alone will express what I mean in this discussion, yet since I can not conveniently use both terms, I shall restrict myself to "image" and hope that its meaning may be extended to include the other, and in its broadest sense.) Fig. 1 will serve for the present as a visual representation of the mind and its contents.

Very likely, at any given instant, at least four of the five symbol images are faded from consciousness (since it

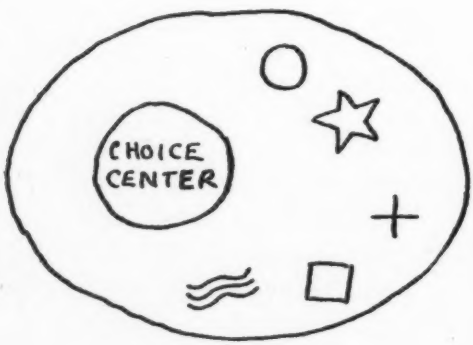


FIG. 1

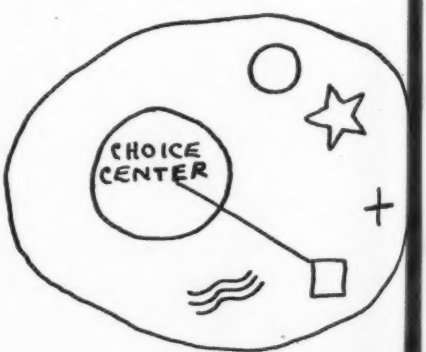


FIG. 2

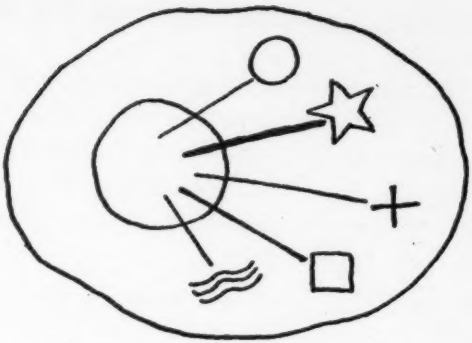

FIG. 3

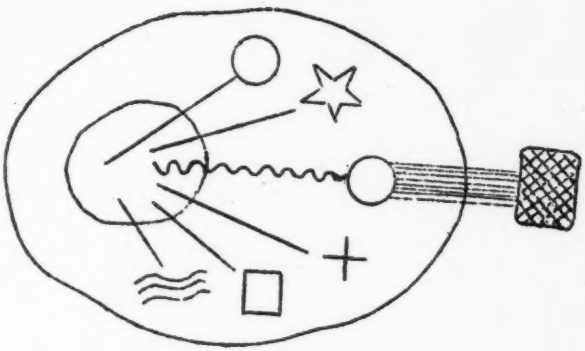

FIG. 4

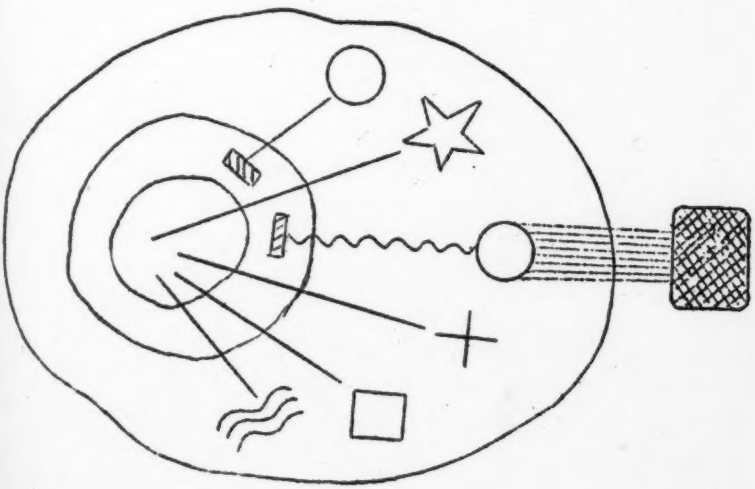

FIG. 5

appears impossible to be aware of more than one of them at a time). Yet all five images have special strings to them, as it were (since the conscious set demands it), and each can be drawn up into consciousness instantly at will, or can be jerked up with particular facility by any associative train that may encounter it.

Fig. 2 depicts this simple situation during a fleeting instant. Four of the five images are below the conscious level. But the square, touched by an associative train perhaps, has emerged into relative consciousness; it possesses a certain intensity or illumination that has an inevitable effect upon the choice center. The line between the two represents both this effect and its reaction; i. e., if the line is considered as having a direction from the image to the choice center, it can be called a "line of effect" or an "effect-line," but if the direction of the line is regarded as reversed, it becomes a "line of attention."

So much, then, for this simplest of situations—a single accentuated image, a single line of effect; it would seem that but a single choice is possible, and as far as this situation is concerned, it is.

The choice center, however, has the faculty of memory, and assuming that not one but several images have appeared and disappeared, it can retain their effects long enough to make a choice between them. As far as subjective awareness can determine, the number of images appearing to the choice center previous to an individual call would seem to be seldom more than two or three, even in slow calling. To take the widest-choice situation, however (as would undoubtedly occur in *very* slow calling, at least), let us assume that all the images have appeared once or more, with varying degrees of illumination or intensity, which in turn have determined the strength of the effect-attention lines, as shown in Fig. 3. No doubt the star is the image that is chosen.

This covers the situation where a subject is *simply calling symbols.* Guesswork of any kind is completely absent,

especially as there is nothing to be guessed. As this situation, together with the mechanism involved and the results produced, is of vital importance to an understanding of below-chance scoring—and indeed, of ESP generally—I shall name it the "basic calling process," and have occasion to refer to it later on.

But supposing that the subject, instead of calling the symbols idly and with no purpose, now attempts to guess the sequence order of a pack of cards. It does not necessarily follow that any new factor whatever will be introduced. The symbols may be called as before, and the same associative trains may continue to determine what calls are made.

It is apparently possible, however, that a new factor will appear, an ES (extra-sensory) factor. Assuming that a face-down card before the subject is a circle, his choice center may receive a sudden and different sort of impression altogether—an impression of a circle image *that is not the circle image sometimes making itself felt in the basic calling process.* For there is reason to believe that an ES effect-line comes from an image all its own, an image that has been suddenly projected, as it were, from the object itself into the mind. Accordingly it can be assumed that there are now six instead of five images lurking in the mind, and capable of making an impression upon the choice center.

At first glance there may appear to be no evidence for this conclusion, and if the reader should think so, I can only point out that providing ESP is accepted as a fact, some at least of the many public reports of it must be true, and these include many accounts of experiences where *totally unknown* objects have been perceived—such as a strange house in an unvisited country, a human face that is not encountered until later, or an original drawing made secretly by someone at a distance. It must follow from this, it seems obvious, that any ES image appearing in the mind comes directly from some object (when it is an object that is concerned), and should be differentiated from any similar "normal" image that the mind may already hold.

This is a key point in the present argument, and one that is hardly disputable in the face of the great number of well-attested cases (many in the annals of the S.P.R.) where individuals, by telepathy, clairvoyance, psychometry, etc., have "perceived" facts of which they were previously in total ignorance, and which hence would be dissimilar to anything already in the mind.

The principle is illustrated in Fig. 4, the twisted line being, of course, the ES effect-line, which when strong enough prevails upon the choice center, and results in an absolutely and inescapably correct "guess." This is the event that by its frequency determines the extent of positive ESP ability manifested by a subject.

The ground is now prepared to allow the illustration of blocking or rejection, the process by which below-chance scoring is explained. In Fig. 5 the situation is the same as in Fig. 4—except that a blocking factor suddenly appears. Its incidence is unmistakable. It stops the course of the twisted ES effect-line, *and at the same time severs the effect-line attaching to the corresponding normal image.* It is as though, for the moment, all impression of a circle (ES *or otherwise*) were completely erased, leaving the subject only four images from which to make a choice. In other words, for that particular choice *he must be wrong,* and could not possibly be otherwise.

Although the mechanical nature of this representation may be disputed, yet assuming it will serve to state the case, there is no other possible explanation, I think, of what occurs.

If we return to the deliberate below-chance scoring of Pearce and Stuart it will appear that in rejecting an ES impression they would also reject, automatically, the corresponding normal image. It can probably be assumed that they would be unable to distinguish between the two.

The variations of consciousness in the choice center now need to be considered. (Distinction should be made between the choice itself, and the process determining it. The choice

may be conscious when the process of choosing is not. It is the latter I am now concerned with.)

At times the center functions in a highly conscious way, as when a man is deciding where to go on his vacation. If the same man, however, were asked to name as quickly as possible four of his acquaintances, the action of his choice center would appear to be less conscious. And in the rapid calling of ESP symbols it is perhaps less conscious still.

So far, however, we have not left consciousness very far behind. If the naming of acquaintances or ESP symbols is slowed down, more consciousness immediately surges into the picture, indicating that it was never far away. The action of blocking, however, seems to take place on quite a different level, and the conscious mind has no clue whatever as to what is going on. This level would be subconsciousness proper, and in Fig. 5 I have represented it by a cortical area. In previous figures this has been omitted.

The many criticisms that these diagrams invite cannot be answered. My purpose has been merely to illustrate a simple principle, irrespective of all else.

Blocking is a most remarkable phenomenon, with many important implications, as will be seen. For instance, it is obvious that the subconscious choice center can differentiate between an ES image and a normal one; for it is the very appearance of the ES image that begins the process. It is the ES image that is blocked *first,* and the corresponding normal image is only blocked, apparently, because it bears a resemblance to the other.

It is as though there were, at times, something so distasteful to the subconscious choice center about an ES *effect,* that the distaste is carried over to the image that it springs from, and carried further to apply to any similar image existing in the mind.

For it is obviously not the circle *in itself* that is rejected. There is no reason why the normal circle-image, left to itself, should not lie in the mind peacefully enough. And no doubt even the ES circle, *as a circle,* is not repellent to the

choice center. We can only suppose, then, that any ES image has a special character of its own, instantly recognized by the subconscious, and at times instantly rejected.

To recapitulate, it appears that there are several steps constituting ESP proper—and to clarify these, new terms are necessary.

*First*: The appearance in the subconscious of an image that did not arrive via the ordinary senses. Let me call this a "direct" image (heretofore called an ES image) in contradistinction to a "normal" one—and let the mere arrival of such a direct image be designated as "subconscious awareness" or "SCA."

*Next*: The treatment accorded the direct image. It may be blocked or rejected, or it may be allowed passage into relative consciousness. In the latter case the image becomes merely *available* to consciousness; it is not necessarily chosen by the choice center or called. As a term to represent this passage of a direct image into relative consciousness I suggest "penetration."

*Finally*: Having by penetration arrived at a near-conscious level the direct image co-exists with various normal images. Relative to them it may be "dominant" or "subdominant." If dominant, it is chosen; subdominant, it is not.

It is now possible to express the whole process in the form of an equation: *SCA + penetration + dominance = ESP.* Fig. 6 is an attempt to visualize this.

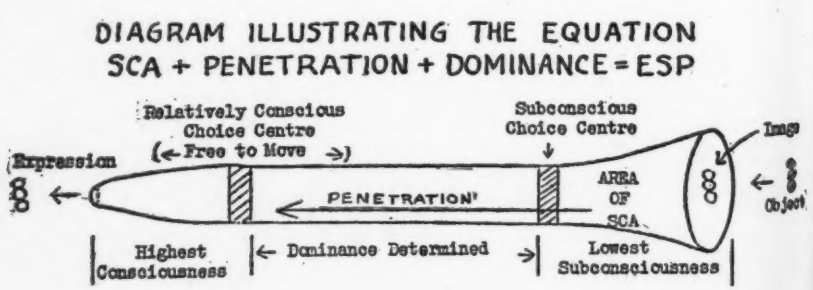


FIG. 6

If there is any truth in Fig. 5 and my reasoning, it follows that the subconscious can:

(1) Distinguish a direct image from a normal one.
(2) Accept or reject an image.
(3) Recognize the similarity between two images and, disliking one, reject them both.

Considering the generally unknown nature of the subconscious, such indications should hold some interest. The fact that the subconscious can distinguish a direct image from a normal one appears the most significant.

It is now clear that without SCA (subconscious awareness) there can be no blocking—and this is a truth depending on no diagram. To reintroduce the diagram, however, the direct image must actually gain the attention of the subconscious choice center before it can inspire the distaste that will result in a similar normal image being blocked as well.

Let us now ask: what are the conditions under which blocking most frequently occurs? They have been demonstrated to be frustration, anxiety, annoyance, and related states of mind. But such states of mind, it may well be emphasized, are the very ones that have been believed to inhibit ESP, and often to prevent it altogether.

***How is it, then, that blocking, which undoubtedly depends upon SCA, also depends upon the very conditions that inhibit ESP, of which SCA is a factor?***

There can be only one answer—that the conditions mentioned, although truly inhibiting ESP, do not inhibit that process *in its entirety,* but only break the sequence of events that constitutes it. Penetration alone is weakened or made impossible, but SCA itself is quite unchanged. *From this it follows that SCA is probably a constant in every individual, and that the blocking or rejecting factor is the only variable one.* But this conclusion may seem more reasonable if I lead up to it by other steps.

It will be recalled that in blocking, Fig. 5, the direct image is so distasteful to the choice center that the distaste and

subsequent rejection are carried over to the corresponding normal image. Now, experimental evidence of blocking is relatively rare; many more experiments show a degree of ESP than give any indication of blocking at all, and furthermore, degrees of blocking that are encountered are generally much less than degress of ESP manifested by the same individual at other times.

It can be asked, then, if blocking, being so unusual, does not represent an *extreme degree* of distaste engendered in the choice center. May there not exist lesser degrees of distaste and consequently lesser degrees of blocking than that illustrated in Fig. 5—degrees that would result in the rejection *merely of the direct image,* and not of the corresponding normal image at all? If this can be assumed, the result would *appear* merely as a simple decrease of ESP, and the underlying significance might never be suspected. This reasoning brings us again, then, to the conclusion already reached—that SCA is a constant, and that ESP derives its variability from the penetration factor, which is variable.

If we retrace our steps from here, blocking can be seen in a much clearer light.

A certain subject, let us say, possesses a SCA factor that is constant at 10 percent—which is to say that during 10 percent of the time he is seeking to guess the identity of an unknown thing, his subconscious choice center is receiving direct images of the object.

Supposing he calls 1000 cards in 1000 seconds. We know that he is going to call approximately 200 of these right by chance. But during 10 percent of the 1000 seconds (or during 100 calls) his SCA factor will be operative, and will result in his subconscious choice center being aware of (I do not say accepting) 100 absolutely correct images.

It does not follow, of course, that he will make 300 hits in the 1000, or an average score of 7.5—for of the 100 direct images making themselves felt, some or all may be instantly

rejected. Supposing half are rejected; the remaining half are accepted, and, providing they are dominant, are voiced as calls, inevitably correct ones. This would give 50 correct ESP calls plus 200 correct chance calls—a total of 250 in the 1000, or an average score of 6.25. (For simplicity I ignore a certain overlapping that exists here.)

But perhaps, again, all of the 100 direct images are rejected. The subject's scoring immediately drops to chance. Now this in a sense is the maximum degree of rejection, and accordingly the maximum degree of distaste might be defined as that which would reject all incoming direct images. But such a definition would be wrong. After all, there is no reason to suppose that the distaste for direct images might not increase past the point where all direct images were rejected. This would result in what might be called an accumulated potential of distaste, which might very well attach to similar normal images existing in the mind.

Thus is blocking apparently explained—blocking in the sense that I originally introduced it. But since blocking or rejection most commonly applies to direct images only, let the terms henceforth have this limited application; and in cases where both the direct and the corresponding normal image are rejected, let the action be differentiated from the simpler one by the terms "multiple-blocking" or "multiple-rejection."

This explanation of multiple-blocking, incidentally, indicates why it is so limited in degree and so infrequently encountered; simple rejection of a direct image is one thing, but the rejection of a corresponding normal image involves a *transference* of distaste, a different action altogether, and one which is apparently much more limited in its possible effect.

There exists, I can now point out, an experimental result that may confirm to some extent the present argument. I refer to the "decline curves" that have appeared more than once in ESP investigations. Such curves are roughly typi-

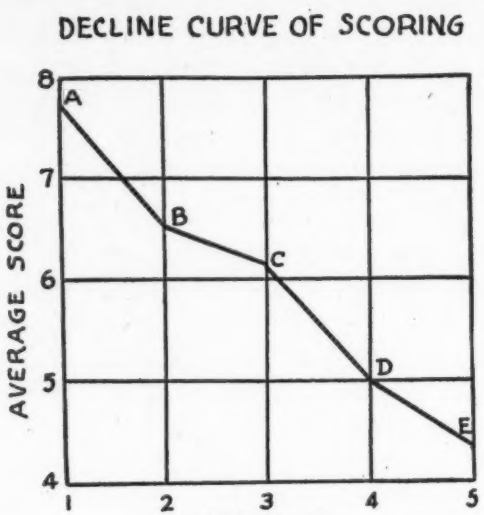


FIG. 7

fied by the one I have drawn in Fig. 7. The subject's scoring, high to begin with, falls off until it reaches the chance expectation, and then (sometimes) declines below it.

The higher scoring, naturally, implies ESP (including SCA). At first glance this appears to decline to zero, and then somehow to reverse itself. But I have pointed out earlier that any actual reversal of ESP is quite impossible. Also I have shown that below-chance scoring absolutely must depend on SCA. It is obvious, then, that SCA is present at points A, B, C, and E. Are we to suppose it has disappeared at D? It seems more reasonable to assume that it is present at all five points. Thus is indicated again the constant character of SCA, its presence in chance scoring as well as in the highest ESP performances.

A close and interesting resemblance exists between the ESP process and a process which is inseparable from ordinary conscious life. I refer to suggestion and the reactions it involves. For example, no one is immune to receiving suggestions, but the final effect of these depends on the ex-

tent to which any given individual accepts them. Some individuals are particularly "open to suggestion"; others, less so, succeed in rejecting a large part of all suggestions making themselves felt. A third class seem to find suggestions so distasteful that they react "negatively" to them—that is, they reject even their own ideas when these correspond to suggestions coming in, an action commonly known as perversity or contrariness.

ESP variations, then, may be variations of suggestibility on a subconscious level, the difference between conscious and subconscious suggestions being that those received by the subconscious are always "right." (Just what "right" means, in different cases, brings up so many problems I will not attempt to investigate them here. Where subconscious suggestions relating to cards are concerned, it means simply that the suggestions—direct images—are of the actual symbols; but in matters of general evaluation and decision the question of what—if anything—is offered to the subconscious plunges one into depths of speculation that cannot be explored in this report.)

To ignore my whole thesis to this point, I suppose an alternative hypothesis might be that the mind was in the line of fire of both right *and wrong* ES suggestions. But this would imply a conscious source—a possibility that has obviously too many ramifications to be considered here.

The question remains of how large the SCA constant may be in any individual. Is his subconscious choice center receiving direct images 10 per cent of the time?—or 5 per cent?—or 20 percent? This would be difficult to determine, since blocking presumably can be operative to *any* extent, and hence could effectively conceal even a large degree of SCA. Accordingly, the most natural assumption is that SCA is constant at 100 percent, or operative all the time—which would mean that the subconscious choice center is bombarded with correct information (whatever that may be) whenever attention is directed to any given problem; or, in other words, that it is in constant touch with knowl-

edge not ordinarily available to the conscious mind because it is subconsciously rejected.

## The Experiments

In 1937, upon learning of Rhine's work, I began some similar experiments of my own. As the manner of their performance will be considered important, I shall outline it.

My earliest experiments, aimed, as they all were, at above-chance scoring, were conducted informally, as a novelty, and without success. Records of them were destroyed. But after making a preliminary 10 or 15 runs through the pack, and becoming seriously interested, I started keeping permanent records, and at the same time established the practice of carefully checking all results. In addition I paid attention to adequate shuffling—ten shuffles or more before each run—and invariably cut the deck as a routine precaution before beginning. Once started, the records were continuous, and contained in sequence the results of all experiments.

In ESP work, involving the ESP pack, it is possible to test specifically for clairvoyance, for telepathy, and for precognition. As I worked entirely alone, the telepathic techniques were impractical. The idea of testing precognition did not occur to me—at least not until later, at which time I did not care to change my methods. There were accordingly left to me those methods which tested clairvoyance, and I made use of two of these.

The DT (down through) method consists of shuffling the pack, cutting it, and leaving it in sight on the table, whereupon the subject writes down 25 symbols in the order in which he guesses the cards in the deck are arranged, beginning with the top. His symbols are then compared, one by one, with the cards themselves, and his hits are marked, totalled, and recorded.

In the OM (open matching) method, five different cards from a spare deck are laid out face upwards on the table. A complete deck is then shuffled and cut, and the subject distributes the cards, face downwards, into five piles below

the designating key cards, in accordance with his impression of where each card belongs. At the end of the run those cards which are found in appropriate places are considered hits.

I began with the DT technique, and after using it for 17,000 trials switched to the OM method and stayed with it. The occasion for the change in method was a change in the cards used. For the first 17,000 trials I used homemade decks consisting of plain visiting cards upon which the symbols were drawn lightly in ink. The cards were uniform, heavy, and opaque, and no trace of the symbols could be discerned from the backs—unless, perhaps, the individual cards were deliberately held up to a strong light. After completing the first 17,000 trials (April, 1938) I procured some decks of standard printed ESP cards, colored, and thereafter used them, changing my method at the same time from DT to OM.

This dual change, involving both cards and method, may be noteworthy, for of all my 60,000 trials the third group of 10,000 (i.e., those numbered from 20,000 to 30,000) are definitely the most interesting, as will be seen.

Upon completing 30,000 trials I made several analyses of the results and circulated a privately published report of the work (July, 1939). I was dissatisfied, however, with the limitations inherent in the experiments—the fact that they were unwitnessed, and that they depended upon a single subject. I therefore told the story of my difficulties to a few friends, and from several offers of assistance accepted the offer of R——, whose work, I felt confident, would be both conscientious and accurate. I instructed R—— in all the details I habitually observed, and suggested the OM method, which was used. While I performed my second lot of 30,000 trials, R—— performed 60,000, which thus gave me two groups of 60,000 trials each for analysis—the basis of a second published report (July, 1941), and of the present paper.

Although the experiments in each case were performed

privately, and without many of the precautions now deemed advisable, I hope the fact that my own mass of experiments has been entirely and independently duplicated will take the place, to some extent, of the omitted precautions, such as screening of the cards, independent checking, etc.

From the beginning my own experimental results, according to all ordinary standards, were entirely without significance, and it may hence be surprising that I took the trouble to obtain so may. The explanation lies in my hypothesis: I was deterred from stopping by the conjecture that although a subject might call, say, 10,000 cards, and might obtain an average score of exactly 5, factors other than chance might nevertheless be operative, and might possibly be revealed by a close examination of the individual scores. For example, assuming the existence of multiple-blocking on the one hand, and of "unblocking" (a term which may be more appropriate than ESP) on the other, I doubted if these two functions could remain in such perfectly balanced opposition as to escape detection in so-called chance scoring. It was soon apparent, however, that if these functions were to be detected at all, a great amount of experimental data would be necessary, which accounts for my persistence.

It may also be suggested that since my eventual purpose was to investigate so-called chance results, my motivation in the experimental work might thereby have been affected, i.e., I might have attempted to obtain high and low scores alternately, to support my theory. In answer to this I will say that my ESP ability, as a definite or measurable thing, was from the beginning *nil,* and hence not subject to manipulation. I did feel, however, that at times the scoring persisted above chance, and at times below; but it was apparent that *if* this was the case, *both* types of scoring resulted from high-trying. It seemed that when the scoring appeared consistently low, any effort to raise it only lowered it still further. Accordingly there was no need, even had I wished to balance the results, of any low-trying.

In the light of later analyses, and the long high and low

runs possible by chance, I now consider it unlikely that any significant up or down phases in my own scoring would be recognizable by sight.

To return to the figures, my own 60,000 trials yielded 12,011 hits as against the expected 12,000, an average of 5.005 per run of 25. R——'s hits totalled 12,258, gave an average of 5.1075 and a critical ratio of 2.6 which probably indicated a slight degree of ESP that was unfortunate for my purposes; this could not be helped, however, and I have constantly borne it in mind when evaluating results.

In addition to these two groups of 60,000 "experimental" trials, I obtained the results of 120,000 "mechanical" trials —scores procured by methods designed to exclude any mental factor whatever, and which hence would constitute chance results in a pure form. Dr. Rhine kindly provided me with the bulk of these mechanical scores. They totalled 23,895, gave an average of 4.978 and a critical ratio of –0.8. I wanted them as a rough "control" for various investigations. The best measure, of course, for experimental figures is mathematical expectation, which I make use of in addition to the necessarily approximate mechanical yardstick.

I collected all scores, both experimental and mechanical, into basic tables of 100 each, one of which is shown in Table I. In the typing of experimental tables the sequence order of the scoring was strictly followed, and can be observed if the scores are read from left to right across the page. The experimental figures made up into two lots of 24 tables, and the mechanical figures provided 48 tables of presumably chance scores. It was these two groups of 48 tables each that I subjected to analysis, my usual method being to apply the same analysis to both, and to compare the results. This procedure yields a fairly rapid and reliable measure of any inherent difference between the experimental and mechanical tables.

Altogether I made fifteen different analyses, and the work as a whole was characterized by extremely discouraging results. Almost every analysis showed, it is true, a difference

Table I

Representative Table of 100 Scores

One of 48 Experimental Tables

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 7 | 3 | 7 | 6 | 7 | 4 | 3 | 5 | 8 | 5 | 55 |
| 5 | 3 | 8 | 8 | 10 | 5 | 7 | 2 | 8 | 2 | 58 |
| 5 | 5 | 2 | 1 | 5 | 2 | 3 | 6 | 6 | 5 | 40 |
| 5 | 4 | 5 | 5 | 3 | 8 | 5 | 4 | 6 | 7 | 52 |
| 6 | 7 | 6 | 3 | 6 | 6 | 3 | 6 | 2 | 5 | 50 |
| 6 | 6 | 4 | 4 | 5 | 6 | 7 | 7 | 5 | 6 | 56 |
| 4 | 6 | 7 | 3 | 5 | 6 | 5 | 3 | 5 | 2 | 46 |
| 2 | 6 | 6 | 7 | 6 | 6 | 7 | 12 | 3 | 9 | 64 |
| 6 | 9 | 3 | 5 | 7 | 5 | 2 | 6 | 5 | 1 | 49 |
| 3 | 2 | 5 | 4 | 5 | 6 | 9 | 2 | 7 | 5 | 48 |
| 49 | 51 | 53 | 46 | 59 | 54 | 51 | 53 | 55 | 47 | 518 |

between experimental and mechanical figures, but the difference was regularly so small, so nearly infinitesimal, that it seemed impossible, without hundreds of thousands of trials, to establish it with any certainty; indeed, three-quarters of the time I had every reason to doubt whether I was encountering any real difference at all, the similarity between the two types of tables was so nearly perfect. Some of the analyses were complicated, but in the end it proved that the simpler ones were most indicative of a difference; a few of the latter will presently be presented.

But first, the distribution of the table totals may be worth a glance. This is shown in Fig. 8, with the experimental group divided to show R——'s tables and my own (E 1 and E 2 respectively), and the mechanical group likewise divided for comparison. The only noticeable irregularity is in the E 1 group, which contains 17 tables above 500 and only 7 below. Two of the totals, 565 and 569, seem to fall somewhat out of the chance area, the greatest mechanical deviation from 500 being 52 for the table totalling 448. On

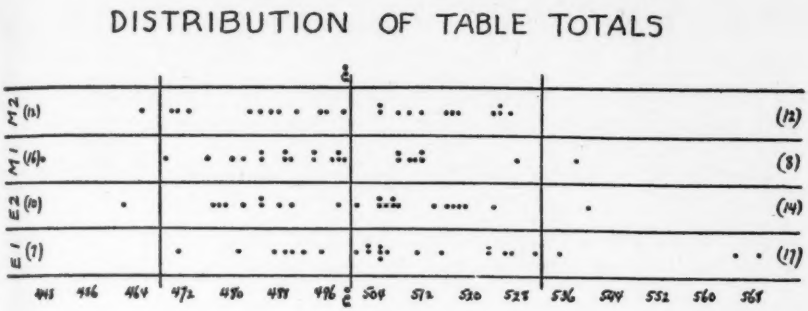


FIG. 8

the whole, however, the distribution of experimental totals seems fairly normal. The actual totals are shown in Appendix I.

Now although it is usually the average score, considered in conjunction with the number of guesses that determined it, that is mathematically appraised, the present study is chiefly concerned with the distribution of individual scores making up the average. Accordingly the mathematical expectations for such individual scores are shown, tabulated in two different ways, in Table II, (a) and (b). It should

## Tables IIa and IIb

## Mathematical Probability

| In guessing 25 cards the chance of getting: | | | | | In 400 runs of 25 cards each, the probability is that there will be: | | | | |
|---|---|---|---|---|---|---|---|---|---|
| none | right | is | 1 | in 264.7 | 2 | scores of | 0 | right | |
| 1 | " | " | 1 | " 42.3 | 9 | " | " | 1 | " |
| 2 | " | " | 1 | " 14.1 | 28 | " | " | 2 | " |
| 3 | " | " | 1 | " 7.4 | 54 | " | " | 3 | " |
| 4 | " | " | 1 | " 5.4 | 75 | " | " | 4 | " |
| 5 | " | " | 1 | " 5.1 | 78 | " | " | 5 | " |
| 6 | " | " | 1 | " 6.1 | 65 | " | " | 6 | " |
| 7 | " | " | 1 | " 9.0 | 44 | " | " | 7 | " |
| 8 | " | " | 1 | " 16.0 | 25 | " | " | 8 | " |
| 9 | " | " | 1 | " 34.0 | 12 | " | " | 9 | " |
| 10 | " | " | 1 | " 84.9 | 5 | " | " | 10 | " |
| 11 | " | " | 1 | " 249.1 | 2 | " | " | 11 | " |
| 12 | " | " | 1 | " 855.3 | 0 | " | " | 12 | " |

be noted, however, that these figures are not always strictly applicable, the reason being that they are based on the assumption that the subject, in naming the symbols, divides his guesses equally among them, calling five of each.

In practice, this even distribution of calls may not occur: the subject's 25 guesses, being made spontaneously and without regard for apportionment, may be divided 6 4 6 4 5, or 7 5 4 3 6, etc. The effect of such a distribution upon the probabilities of scoring has been carefully studied, and it is now agreed that while it involves probabilities somewhat different from those shown in Table II, the discrepancy is not serious, particularly as it is in a direction more likely to weaken than to strengthen the case for ESP.

Briefly, the effect of uneven call distribution is to decrease the probabilities of obtaining extreme scores such as 0's, 1's, 2's, and 8's, 9's, 10's—and at the same time to increase the probabilities of the 4's, 5's, and 6's. The effect is seen clearly in the extreme case: if the 5 5 5 5 5 call distribution were distorted to the maximum, the subject's 25 calls would be all of one symbol, in which case his correct calls would number exactly 5.

In the experiments presented here, the call distribution was variable, ranging from 5 5 5 5 5 to distributions that (rarely) may have been as uneven as 2 3 3 7 10. Since the exact probabilities of uneven distributions are impractical to obtain, I have used the probabilities of Table II as a yardstick, and if these are misleading it is to but a small extent, and the discrepancy can be intelligently allowed for in a last appraisal.

Fig. 9, then—based on Table II—shows the normal curve, the most probable distribution of 400 chance scores. But supposing that instead of chance alone determining the scores, traces of multiple-blocking and unblocking are present in the guesswork. One might then expect to find a distortion of the curve resembling the solid line shown in Fig. 10. Since, in the normal curve, the central area represents the bulk of all scores expected—and also, be it noted,

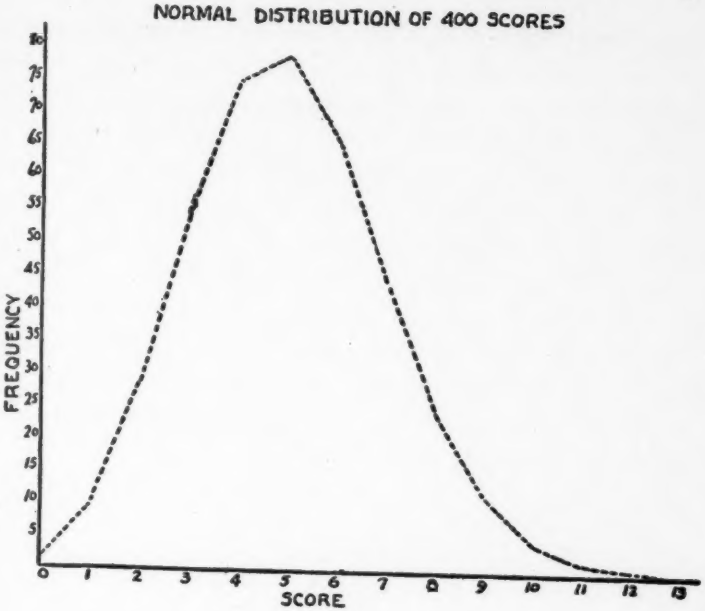
NORMAL DISTRIBUTION OF 400 SCORES
FREQUENCY
SCORE

FIG. 9

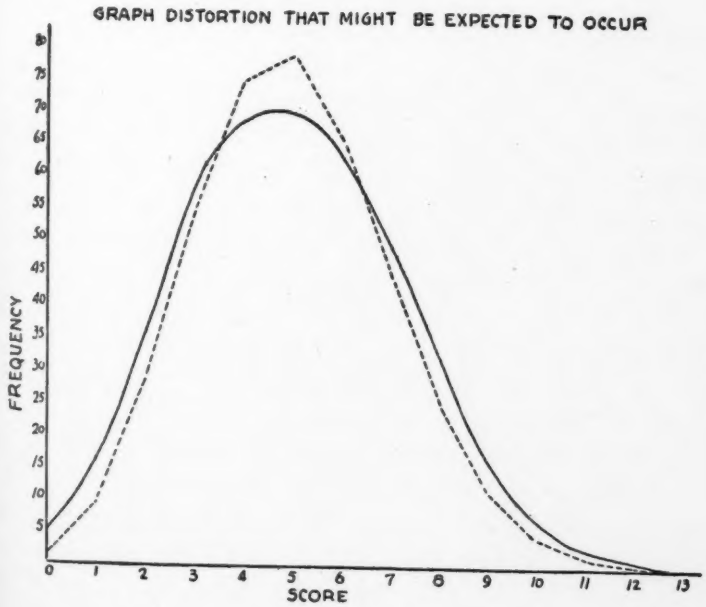
GRAPH DISTORTION THAT MIGHT BE EXPECTED TO OCCUR
FREQUENCY
SCORE

FIG. 10

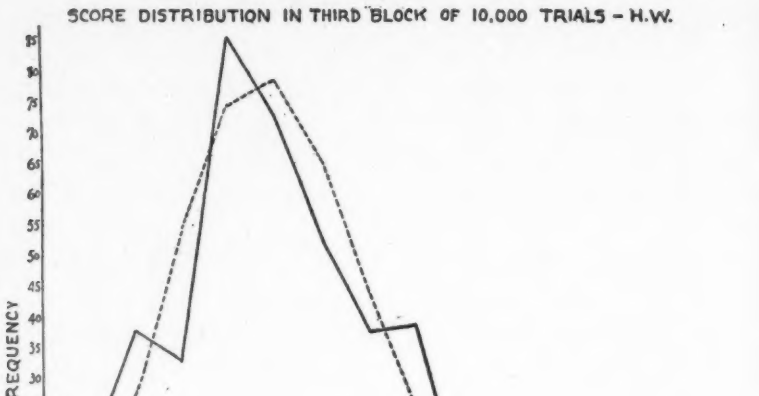


FIG. 11

## Table III

| Score | Expectations per 10,000 trials | Results of 10,000 trials (W 9 - W 12) |
|---|---|---|
| 0 | 2 | 3 |
| 1 | 9 | 18 |
| 2 | 28 | 38 |
| 3 | 54 | 33 |
| 4 | 75 | 86 |
| 5 | 78 | 73 |
| 6 | 65 | 53 |
| 7 | 44 | 38 |
| 8 | 25 | 39 |
| 9 | 12 | 11 |
| 10 | 5 | 6 |
| 11 | 2 | 1 |
| 12 | 0 | 0 |
| 13 | 0 | 0 |
| 14 | 0 | 1 |

represents most of the *time* during which calls would be made—it might be expected that multiple-blocking would revise some of these scores downwards, and throw them to the left. Unblocking, contrarily, would displace others of them to the right. Of course all scores would be affected, but the dislocation of the rarer ones would, collectively, be less, and might be concealed within the major movement.

Early in my work I looked for such distortion, and found it when the results of my third block of 10,000 trials were graphed—solid line in Fig. 11—based on Table III.

For long after I drew this graph, however, I encountered no corroborating evidence of such displacement. The third block of 10,000 was the only one of the six similar blocks to show that particular distortion. Nor did any of R——'s six blocks yield a comparable curve, the chief reason perhaps being that the apparent ESP factor in R——'s work produces, for the most part, a displacement chiefly to the right, and not a balanced distortion as in Fig. 11.

For those who may wish to look at this matter further I include Appendices II and III, giving the score distribution of every table of the forty-eight. Tables 9, 10, 11, and 12 of my own constitute the third block of 10,000 referred to. It will be seen at a glance that the "3" scores are abnormally low; the other deviations are less apparent. The totals show that some trends characterizing tables 9, 10, 11, and 12 persist, although in considerably lesser measure; the 2's, 4's, 5's, 8's, and 9's increase the deviations set up in Fig. 11. I have already mentioned the only explanation of the third block's eccentricity that I can think of.

*Second Analysis.* Referring again to Table I, it will be recalled that the horizontal lines show the order in which the scores were obtained. Also it is to be noted that as a rule a line of scores represents one complete experiment from start to finish. Sometimes two lines were obtained at a sitting, or even three, but it was not often that an experiment terminated in the middle of a line—although in perhaps 20 percent of the experiments this did occur.

Now it may be assumed that if either multiple-blocking

or unblocking were present in the calling, the incidence of these might vary with different experiments, and might at times reveal itself in the "line-totals" on the right. The better to consider these line-totals, I focused attention on the extremes appearing, and defined an extreme total as one of 60 or greater, or of 40 or less. The number of such extreme line-totals per mechanical table I found to be 1.19; the mathematical expectation per table is 1.32; and the number per experimental table turned out to be 1.58. However, this in itself is scarcely significant, especially as half of the 48 experimental tables showed a positive deviation, and hence could possibly be expected to yield a somewhat greater number of extreme scores on that account. For a long time this particular type of analysis lay in abeyance—until one day I became interested in the "vertical column" totals. It was possible, I thought, that these might reveal consistent fluctuations *within* the horizontal lines. The results of this survey were:

Extreme vertical column totals per mechanical table: 1.25
" " " " mathematically expected: 1.32
" " " " per experimental table: 1.75

Broken down for the two experimenters, the last figure gives R—— = 1.75, H. W. = 1.75. This seems to indicate that as a line of scores is begun, added to, and completed, there are concomitant states of mind that have an effect upon the scoring. (As the mathematical expectations involved above may be useful, I include them in Appendix IV).

An interesting thing is the correlation that appears to exist between the number of extreme line-totals and the number of extreme column-totals in individual tables. This is shown for all 96 tables in Appendix V. The total for the mechanical tables is 10 below chance expectation; for the experimental tables, 33 above. It will be noticed that although the experimental tables give nine totals of 7 or over, and the mechanical tables none, the former show, on the other hand, almost as many (32 vs 35) of 3 or less—thus indicating the correlation.

Also of interest is the fact that R——'s slight positive deviation is apparently not misleading for this survey. The bottom half of the experimental figures are from R——'s work; the top half, from my own. Broken down, the 160 extreme totals give 76 and 84 respectively for the two groups of experimental tables involved—the reverse of what might be expected if the higher totals of R——'s tables were to have an effect.

The correlation, if it exists (the evidence has weaknesses), gives a peculiarly good proof of what I was looking for. It corresponds to the customary arithmetic checking of a total —columns added vertically must give a corresponding answer horizontally. As each table represents a rough block of continuous experimenting time, the correlation shows that when multiple-blocking and unblocking reveal their presence in the separate lines of scores, their effect is likewise to be found in small but consistent fluctuations *within* the lines. On the other hand, when multiple-blocking and unblocking are both absent, due to perfunctory calling as in the basic calling process—or when blocking alone exists, rejecting completely the direct images but nothing else—the absence of effect is similarly evinced by two observations that confirm each other.

The apparent correlation can be more easily evaluated if the high extreme-score totals (Appendix V) are considered in conjunction with the table they derive from:

Table IV

| Tables | Extreme-Score Totals | Corresponding Table Totals |
|---|---|---|
| R10 | 8 | 569 |
| R13 | 9 | 565 |
| R14 | 7 | 506 |
| R19 | 8 | 526 |
| R21 | 7 | 531 |
| W6 | 7 | 485 |
| W10 | 7 | 540 |
| W11 | 8 | 501 |
| W12 | 7 | 462 |

In six of the nine table totals the deviations from the expected 500 are relatively great, and appear to account for the extreme scores. Indeed, some connection between the two obviously must exist. But whether the table totals *wholly* account for the extreme-score totals is another question. By referring to Fig. 8, where all table totals are shown, it will be seen that (with two exceptions) mechanical tables show comparable deviations—without, however (Appendix V), comparable extreme-score totals. For comparison, the nine mechanical tables with greatest deviations are shown below.

Table V

| Tables | Extreme-Score Totals | Corresponding Table Totals |
|---|---|---|
| M3 | 4 | 469 |
| M10 | 3 | 538 |
| M17 | 4 | 528 |
| M23 | 6 | 448 |
| M28 | 4 | 470 |
| M35 | 1 | 473 |
| M36 | 1 | 527 |
| M45 | 5 | 471 |
| M46 | 6 | 465 |

Now it is apparent that the excess of extreme line-totals in experimental tables represents really a "flattening" of the line-total graph (here omitted) somewhat comparable to the flattening (except for the peak) that occurs in Fig. 11. This is in the characteristic distortion that appears again and again in different analyses—a preponderance of extreme scores at the expense of central ones—an effect just the opposite, it should be noticed, from the distortion that unequal call distribution, such as 5 6 2 4 8, would account for. And since unequal call distribution was present in the guesswork, and presumably had at least a slight effect, it is safe to say that such evidence as I have presented would, if anything, be somewhat stronger if the calls had always been equally distributed.

*Third Analysis.* As the flattening of the curve of table line-totals was scarcely noticeable, I looked for a method of analysis that would show the effect more strongly, and if possible for all 120,000 experimental trials.

If each table is divided into four blocks by a horizontal and a vertical line (shown in Table I), shorter blocks of scores can be investigated. Each "quadrant" then offers five line-totals of its own, the expectation of which, this time, is 25. The same reasoning that I advanced previous to the presentation of table line-totals again applies; the quadrant line-totals can likewise be expected to reveal the presence of multiple-blocking and unblocking if these are present. The shorter lines of scores have the advantage of representing shorter periods of experimenting time—periods in which the effects of multiple-blocking and unblocking might be more apparent.

Fig. 12 (based on the last two columns of Appendix VI) shows the distribution of quadrant line-totals for my own 60,000 trials (solid line). It will be seen that the curve is indeed flattened relative to expectation (dotted line). The "20" scores to the "29" scores as a group are clearly below chance expectation, and the scores below 20 and above 29 are correspondingly above. From 19 downwards the excess is 28.7 percent, and from 30 upwards it is 19.4 percent.

These transition points at 20 and 29 correspond to similar points in Fig. 11 at 3 and 7, and to points in the table line-total distribution at 42 and 57.

It will also be seen that Fig. 12 is remarkably symmetrical in its distortion. The dips at 20 and 29, almost equidistant from the central 25, are paralleled by two similar dips at 24 and 26, exactly equidistant from the center, and by corresponding dips at 17 and 32, likewise equidistant, or nearly so.

Referring now to Fig. 13, based on Appendix VI and showing R——'s quadrant line-total distribution, it will be seen that the dips at 20 and 29 are again in evidence. The

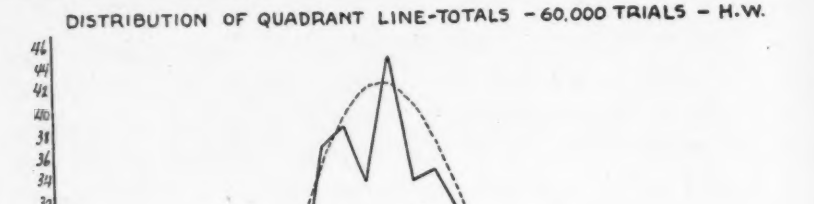
DISTRIBUTION OF QUADRANT LINE-TOTALS – 60,000 TRIALS – H.W.
FREQUENCY
SCORE

FIG. 12

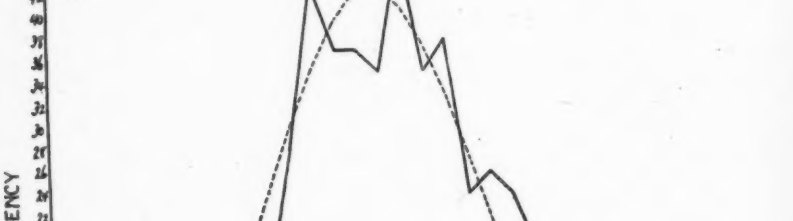
DISTRIBUTION OF QUADRANT LINE-TOTALS – 60,000 TRIALS – R –
FREQUENCY
SCORE

FIG. 13

curve as a whole, however, is far less symmetrical than that of Fig. 12. The displacement to the right, characteristic of R——'s work, is obvious. The scores from 30 upwards are 27.4 percent in excess of expectation (as against my own 19.4 percent excess), and the scores from 19 downwards are 12.3 percent *below* expectancy. If the displacement to the right is imaginatively corrected (to discount for the ESP factor that presumably causes it) an excess of both high and low extreme scores will result, roughly comparable to the distribution in Fig. 12. The excesses of the two graphs are tabulated in Appendix VII.

This completes the present evidence in support of the hypothesis. It appears that even in so-called chance scoring (*vide* my own over-all average of 5.005, and R——'s of 5.1075) multiple-blocking and unblocking nevertheless exist. A curiously good example of their incidence appears in an experiment reported by Rhine in *New Frontiers of the Mind* (Farrar & Rinehart, 1937), pages 207 and 208, quoted below.

"After deliberately stopping Pearce in the midst of his finest scoring at 100 yards, we increased the distance to 250 yards. At the same time it was necessary for Pratt himself to go to another room, and he took the cards to be used to the Duke Medical Building. Then something happened to Pearce's scoring. It was not a falling off due to distance, apparently, since in his first day's work he scored 12 and 10 per 25, which was right up to his best average. But the next day he got exactly chance. The third day he scored high again with two 10's; then the fourth day it was only a 2 and a 6, the fifth day a 5 and a 12, down again the next day to 7 and 5, and up once more to make it a perfect zigzag. But his total average for this block of work was only 6.7.

"As to what started this strange alteration of high and low scoring neither Pearce nor we had or have any reliable information. In the course of twenty-two days' work, or forty-four runs, he scored zero three times. By chance alone zero would not be expected even once. On the other hand,

he scored 10 or above in thirteen out of the forty-four trials. By chance, no score as high as that would be expected more than once. In three successive days he made the score of 4 five times out of the six runs, the other score being a 1.

"This scoring is the most peculiar that Pearce ever did. It indicates that he was almost alternately successful and unsuccessful, but when he was at his best he did quite as well as, in fact better than, when he was in the same room with the cards. When he was having a poor day he did worse than chance."

This performance, of a subject possessing high ESP ability, reveals multiple-blocking and unblocking strongly contrasted. If such cases were more frequent they would provide valuable opportunity to investigate the mechanism further.

As I have said before, most of my analyses have been omitted, some because they were almost barren of results, and others because time has not been available to complete the work. There is much to be learned, I think, from a further investigation of such data as have been studied.

In conclusion I warmly express my thanks to those who have contributed encouragement or assistance. I am indebted to Professor J. B. S. Haldane, Dr. R. H. Thouless, and Mr. Whately Carington for detailed analysis and criticism of my second report. My thanks are also due to Dr. Rhine for books, pamphlets, and encouragement in the work. Particularly I wish to thank R—— for much generous assistance, both in the experiments and in the work of calculation.

* * *

*Author's Note*: The great majority of contemporary psychologists, psychiatrists, neurologists, and other scientists are yet to be convinced that ESP actually exists. This prevailing orthodoxy, to a believer in ESP, constitutes a major obstacle to human progress; for if the great army of thinkers in the world could once be persuaded to take a serious

interest in such evidence as appears in this JOURNAL, there is little doubt that their subsequent investigations would soon lead to important advances in human knowledge.

The one thing most responsible for deterring scientists from the field has been the fact that, to date, ESP evidence has proved difficult or impossible to reproduce—with the result that the few orthodox scientists who undertake to duplicate the experiments often report negative results. As such failures to confirm ESP evidence are as a rule widely circulated, the public (both lay and scientific) are accordingly encouraged in their skepticism.

Perhaps the most consequential aspect of this report then, is that the procedure used gives promise of being one which will invariably yield results. If it turned out that 60,000 calls by any normal person always showed, on analysis, unaccountable distortions similar to those reported, it would constitute a challenge that the orthodox scientists could not well ignore, and the millennium that ESP workers hope for —a widespread and active interest in their field—would then be near at hand.

# Appendix I

## Table Totals

| | | | | | |
|---|---|---|---|---|---|
| W | 1 | 517 | Mechanical | 1 | 482 |
| W | 2 | 505 | | 2 | 494 |
| W | 3 | 508 | | 3 | 469 |
| W | 4 | 478 | | 4 | 497 |
| W | 5 | 516 | | 5 | 498 |
| W | 6 | 485 | | 6 | 490 |
| W | 7 | 519 | | 7 | 511 |
| W | 8 | 518 | | 8 | 499 |
| W | 9 | 477 | | 9 | 498 |
| W | 10 | 540 | | 10 | 538 |
| W | 11 | 501 | | 11 | 512 |
| W | 12 | 462 | | 12 | 489 |
| W | 13 | 479 | | 13 | 494 |
| W | 14 | 505 | | 14 | 485 |
| W | 15 | 507 | | 15 | 508 |
| W | 16 | 506 | | 16 | 512 |
| W | 17 | 498 | | 17 | 528 |
| W | 18 | 514 | | 18 | 476 |
| W | 19 | 507 | | 19 | 510 |
| W | 20 | 488 | | 20 | 480 |
| W | 21 | 482 | | 21 | 485 |
| W | 22 | 524 | | 22 | 489 |
| W | 23 | 485 | | 23 | 448 |
| W | 24 | 490 | | 24 | 508 |
| | | 12011 | | | 11900 |

| | | | | | |
|---|---|---|---|---|---|
| R | 1 | 527 | Mechanical | 25 | 488 |
| R | 2 | 505 | | 26 | 510 |
| R | 3 | 503 | | 27 | 525 |
| R | 4 | 471 | | 28 | 470 |
| R | 5 | 487 | | 29 | 525 |
| R | 6 | 489 | | 30 | 512 |
| R | 7 | 490 | | 31 | 505 |
| R | 8 | 481 | | 32 | 496 |
| R | 9 | 505 | | 33 | 518 |
| R | 10 | 569 | | 34 | 483 |
| R | 11 | 495 | | 35 | 473 |
| R | 12 | 535 | | 36 | 527 |
| R | 13 | 565 | | 37 | 487 |
| R | 14 | 506 | | 38 | 508 |
| R | 15 | 501 | | 39 | 505 |
| R | 16 | 492 | | 40 | 495 |
| R | 17 | 523 | | 41 | 485 |
| R | 18 | 503 | | 42 | 499 |
| R | 19 | 526 | | 43 | 517 |
| R | 20 | 505 | | 44 | 524 |
| R | 21 | 531 | | 45 | 471 |
| R | 22 | 511 | | 46 | 465 |
| R | 23 | 523 | | 47 | 516 |
| R | 24 | 515 | | 48 | 491 |
| | | 12258 | | | 11995 |

## Appendix II

## Frequency of Different Scores per Table of 100 Runs

## (2500 trials)

## 24 Experimental Tables (R 1 to R 24)

| | SCORE | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Table | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | |
| R 1 | 0 | 0 | 9 | 10 | 21 | 11 | 24 | 13 | 7 | 3 | 0 | 2 | 0 | 0 | 0 | |
| R 2 | 0 | 3 | 5 | 17 | 16 | 15 | 21 | 12 | 7 | 4 | 0 | 0 | 0 | 0 | 0 | |
| R 3 | 1 | 1 | 5 | 17 | 18 | 14 | 19 | 18 | 5 | 1 | 1 | 0 | 0 | 0 | 0 | |
| R 4 | 0 | 5 | 10 | 17 | 19 | 15 | 14 | 8 | 6 | 4 | 2 | 0 | 0 | 0 | 0 | |
| R 5 | 0 | 1 | 8 | 13 | 20 | 27 | 15 | 8 | 4 | 3 | 0 | 1 | 0 | 0 | 0 | |
| R 6 | 1 | 3 | 9 | 12 | 22 | 14 | 20 | 8 | 6 | 1 | 3 | 1 | 0 | 0 | 0 | |
| R 7 | 1 | 1 | 8 | 13 | 23 | 22 | 11 | 11 | 4 | 3 | 3 | 0 | 0 | 0 | 0 | |
| R 8 | 0 | 4 | 8 | 17 | 20 | 17 | 14 | 8 | 3 | 9 | 0 | 0 | 0 | 0 | 0 | |
| R 9 | 0 | 0 | 4 | 11 | 25 | 25 | 16 | 12 | 4 | 3 | 0 | 0 | 0 | 0 | 0 | |
| R 10 | 0 | 0 | 9 | 2 | 14 | 18 | 30 | 13 | 6 | 3 | 3 | 1 | 1 | 0 | 0 | |
| R 11 | 0 | 3 | 7 | 11 | 21 | 19 | 18 | 13 | 6 | 1 | 1 | 0 | 0 | 0 | 0 | |
| R 12 | 0 | 1 | 11 | 12 | 13 | 16 | 20 | 11 | 8 | 2 | 2 | 3 | 1 | 0 | 0 | |
| R 13 | 0 | 3 | 6 | 11 | 13 | 14 | 14 | 15 | 17 | 3 | 2 | 1 | 1 | 0 | 0 | |
| R 14 | 1 | 2 | 6 | 18 | 14 | 19 | 21 | 5 | 6 | 5 | 1 | 1 | 1 | 0 | 0 | |
| R 15 | 0 | 3 | 3 | 19 | 20 | 17 | 21 | 5 | 3 | 5 | 4 | 0 | 0 | 0 | 0 | |
| R 16 | 0 | 2 | 6 | 16 | 19 | 23 | 16 | 7 | 7 | 2 | 2 | 0 | 0 | 0 | 0 | |
| R 17 | 0 | 4 | 5 | 8 | 17 | 25 | 14 | 16 | 6 | 3 | 1 | 1 | 0 | 0 | 0 | |
| R 18 | 0 | 2 | 7 | 12 | 17 | 22 | 19 | 12 | 7 | 1 | 1 | 0 | 0 | 0 | 0 | |
| R 19 | 1 | 3 | 13 | 6 | 12 | 16 | 18 | 15 | 9 | 4 | 3 | 0 | 0 | 0 | 0 | |
| R 20 | 0 | 2 | 3 | 19 | 17 | 21 | 15 | 12 | 7 | 3 | 1 | 0 | 0 | 0 | 0 | |
| R 21 | 1 | 6 | 5 | 7 | 16 | 18 | 18 | 11 | 13 | 1 | 2 | 2 | 0 | 0 | 0 | |
| R 22 | 0 | 3 | 7 | 11 | 20 | 16 | 22 | 9 | 5 | 5 | 1 | 1 | 0 | 0 | 0 | |
| R 23 | 0 | 2 | 3 | 11 | 15 | 28 | 21 | 11 | 4 | 3 | 2 | 0 | 0 | 0 | 0 | |
| R 24 | 0 | 0 | 8 | 6 | 27 | 20 | 21 | 6 | 6 | 3 | 3 | 0 | 0 | 0 | 0 | |
| | 6 | 54 | 165 | 296 | 439 | 452 | 442 | 259 | 156 | 75 | 38 | 14 | 4 | 0 | 0 | total |
| | 9 | 57 | 170 | 326 | 448 | 470 | 392 | 266 | 150 | 71 | 28 | 10 | 3 | 0 | 0 | expctd. |

## Appendix III

### Frequency of Different Scores per Table of 100 Runs (2500 trials)

### 24 Experimental Tables (W 1 to W 24)

| | SCORE | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Table | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 |
| W 1 | 0 | 0 | 6 | 9 | 22 | 25 | 17 | 9 | 9 | 2 | 1 | 0 | 0 | 0 | 0 |
| W 2 | 0 | 4 | 8 | 15 | 17 | 15 | 16 | 10 | 8 | 5 | 1 | 0 | 1 | 0 | 0 |
| W 3 | 0 | 0 | 8 | 14 | 24 | 16 | 16 | 11 | 4 | 3 | 2 | 2 | 0 | 0 | 0 |
| W 4 | 2 | 1 | 11 | 17 | 17 | 14 | 18 | 8 | 8 | 2 | 2 | 0 | 0 | 0 | 0 |
| W 5 | 0 | 0 | 8 | 13 | 21 | 16 | 18 | 9 | 11 | 3 | 0 | 1 | 0 | 0 | 0 |
| W 6 | 1 | 2 | 7 | 14 | 22 | 21 | 12 | 13 | 3 | 3 | 2 | 0 | 0 | 0 | 0 |
| W 7 | 0 | 2 | 2 | 13 | 17 | 24 | 23 | 9 | 7 | 1 | 2 | 0 | 0 | 0 | 0 |
| W 8 | 0 | 2 | 10 | 12 | 7 | 24 | 22 | 13 | 5 | 3 | 1 | 0 | 1 | 0 | 0 |
| W 9 | 2 | 3 | 11 | 9 | 24 | 19 | 8 | 10 | 10 | 4 | 0 | 0 | 0 | 0 | 0 |
| W 10 | 0 | 5 | 7 | 6 | 17 | 13 | 19 | 14 | 14 | 2 | 3 | 0 | 0 | 0 | 0 |
| W 11 | 1 | 4 | 7 | 9 | 22 | 21 | 16 | 7 | 6 | 4 | 2 | 0 | 0 | 0 | 1 |
| W 12 | 0 | 6 | 13 | 9 | 23 | 20 | 10 | 7 | 9 | 1 | 1 | 1 | 0 | 0 | 0 |
| W 13 | 1 | 1 | 11 | 17 | 19 | 15 | 14 | 11 | 8 | 1 | 2 | 0 | 0 | 0 | 0 |
| W 14 | 0 | 5 | 1 | 15 | 20 | 16 | 19 | 16 | 6 | 1 | 1 | 0 | 0 | 0 | 0 |
| W 15 | 1 | 2 | 4 | 14 | 20 | 25 | 12 | 8 | 8 | 4 | 1 | 0 | 1 | 0 | 0 |
| W 16 | 0 | 1 | 8 | 17 | 17 | 15 | 17 | 13 | 8 | 2 | 2 | 0 | 0 | 0 | 0 |
| W 17 | 0 | 2 | 6 | 17 | 23 | 14 | 14 | 13 | 5 | 5 | 0 | 1 | 0 | 0 | 0 |
| W 18 | 0 | 2 | 7 | 15 | 17 | 17 | 20 | 8 | 9 | 1 | 2 | 1 | 1 | 0 | 0 |
| W 19 | 0 | 3 | 6 | 13 | 21 | 20 | 12 | 14 | 5 | 2 | 3 | 1 | 0 | 0 | 0 |
| W 20 | 0 | 3 | 12 | 13 | 16 | 18 | 13 | 15 | 7 | 2 | 0 | 1 | 0 | 0 | 0 |
| W 21 | 0 | 5 | 6 | 17 | 22 | 16 | 14 | 10 | 5 | 2 | 2 | 0 | 1 | 0 | 0 |
| W 22 | 0 | 3 | 8 | 11 | 15 | 22 | 16 | 8 | 10 | 4 | 1 | 1 | 1 | 0 | 0 |
| W 23 | 1 | 3 | 10 | 10 | 25 | 16 | 13 | 10 | 8 | 1 | 2 | 1 | 0 | 0 | 0 |
| W 24 | 0 | 2 | 9 | 16 | 18 | 14 | 21 | 13 | 3 | 2 | 1 | 1 | 0 | 0 | 0 |
| | 9 | 61 | 186 | 315 | 466 | 436 | 380 | 259 | 176 | 60 | 34 | 11 | 6 | 0 | 1 total |
| | 9 | 57 | 170 | 326 | 448 | 470 | 392 | 266 | 150 | 71 | 28 | 10 | 3 | 0 | 0 expctd. |

## Appendix IV

## Expected Distribution of Line-Totals (Hits per 250 Trials) Resulting from 30,000 Trials

(Calculated from individual probabilities—not by Binomial Theorem)

| *Score* | *Expectation* | *Score* | *Expectation* |
|---|---|---|---|
| 25 | .001 | 51 | 7.407 |
| 26 | .002 | 52 | 7.088 |
| 27 | .005 | 53 | 6.619 |
| 28 | .009 | 54 | 6.036 |
| 29 | .018 | 55 | 5.379 |
| 30 | .033 | 56 | 4.682 |
| 31 | .058 | 57 | 3.984 |
| 32 | .100 | 58 | 3.314 |
| 33 | .165 | 59 | 2.697 |
| 34 | .263 | 60 | 2.145 |
| 35 | .406 | 61 | 1.671 |
| 36 | .606 | 62 | 1.273 |
| 37 | .876 | 63 | .950 |
| 38 | 1.228 | 64 | .694 |
| 39 | 1.669 | 65 | .497 |
| 40 | 2.201 | 66 | .348 |
| 41 | 2.819 | 67 | .239 |
| 42 | 3.494 | 68 | .161 |
| 43 | 4.240 | 69 | .106 |
| 44 | 4.983 | 70 | .068 |
| 45 | 5.709 | 71 | .043 |
| 46 | 6.356 | 72 | .027 |
| 47 | 6.901 | 73 | .016 |
| 48 | 7.295 | 74 | .010 |
| 49 | 7.519 | 75 | .006 |
| 50 | 7.557 | 76 | .003 |
| | | 77 | .002 |
| | | 78 | .001 |
| | | | 119.979 |

## Appendix V

## Extreme Line-Totals and Extreme Column-Totals per Table, Showing Correlation

| MECHANICAL TABLES | | | | | EXPERIMENTAL TABLES | | | | |
|---|---|---|---|---|---|---|---|---|---|
| *Table* | *Across* | *Down* | *Tot.* | *Total* | *Table* | *Across* | *Down* | *Tot.* | *Total* |
| M 1 | 3 | 2 | 5 | 00000 | W 1 | 0 | 2 | 2 | 00 |
| 2 | 1 | 0 | 1 | 0 | 2 | 3 | 0 | 3 | 000 |
| 3 | 3 | 1 | 4 | 0000 | 3 | 0 | 2 | 2 | 00 |
| 4 | 1 | 1 | 2 | 00 | 4 | 2 | 4 | 6 | 000000 |
| 5 | 2 | 1 | 3 | 000 | 5 | 2 | 1 | 3 | 000 |
| 6 | 1 | 1 | 2 | 00 | 6 | 3 | 4 | 7 | 0000000 |
| 7 | 0 | 3 | 3 | 000 | 7 | 0 | 2 | 2 | 00 |
| 8 | 1 | 1 | 2 | 00 | 8 | 2 | 0 | 2 | 00 |
| 9 | 0 | 2 | 2 | 00 | 9 | 4 | 1 | 5 | 00000 |
| 10 | 1 | 2 | 3 | 000 | 10 | 3 | 4 | 7 | 0000000 |
| 11 | 1 | 1 | 2 | 00 | 11 | 4 | 4 | 8 | 00000000 |
| 12 | 1 | 1 | 2 | 00 | 12 | 4 | 3 | 7 | 0000000 |
| 13 | 2 | 0 | 2 | 00 | 13 | 1 | 3 | 4 | 0000 |
| 14 | 1 | 0 | 1 | 0 | 14 | 0 | 2 | 2 | 00 |
| 15 | 3 | 1 | 4 | 0000 | 15 | 1 | 2 | 3 | 000 |
| 16 | 0 | 4 | 4 | 0000 | 16 | 0 | 0 | 0 | |
| 17 | 3 | 1 | 4 | 0000 | 17 | 0 | 2 | 2 | 00 |
| 18 | 0 | 0 | 0 | | 18 | 2 | 1 | 3 | 000 |
| 19 | 1 | 0 | 1 | 0 | 19 | 1 | 1 | 2 | 00 |
| 20 | 2 | 1 | 3 | 000 | 20 | 1 | 0 | 1 | 0 |
| 21 | 0 | 1 | 1 | 0 | 21 | 1 | 2 | 3 | 000 |
| 22 | 2 | 2 | 4 | 0000 | 22 | 2 | 1 | 3 | 000 |
| 23 | 3 | 3 | 6 | 000000 | 23 | 4 | 0 | 4 | 0000 |
| 24 | 1 | 3 | 4 | 0000 | 24 | 2 | 1 | 3 | 000 |
| 25 | 0 | 0 | 0 | | R 1 | 2 | 1 | 3 | 000 |
| 26 | 0 | 2 | 2 | 00 | 2 | 1 | 0 | 1 | 0 |
| 27 | 0 | 2 | 2 | 00 | 3 | 1 | 3 | 4 | 0000 |
| 28 | 2 | 2 | 4 | 0000 | 4 | 2 | 2 | 4 | 0000 |
| 29 | 1 | 1 | 2 | 00 | 5 | 0 | 1 | 1 | 0 |
| 30 | 1 | 0 | 1 | 0 | 6 | 0 | 1 | 1 | 0 |
| 31 | 2 | 0 | 2 | 00 | 7 | 2 | 1 | 3 | 000 |
| 32 | 2 | 3 | 5 | 00000 | 8 | 0 | 1 | 1 | 0 |
| 33 | 1 | 1 | 2 | 00 | 9 | 0 | 0 | 0 | |
| 34 | 0 | 1 | 1 | 0 | 10 | 4 | 4 | 8 | 00000000 |
| 35 | 1 | 0 | 1 | 0 | 11 | 0 | 3 | 3 | 000 |
| 36 | 0 | 1 | 1 | 0 | 12 | 1 | 0 | 1 | 0 |
| 37 | 1 | 2 | 3 | 000 | 13 | 4 | 5 | 9 | 000000000 |
| 38 | 1 | 2 | 3 | 000 | 14 | 5 | 2 | 7 | 0000000 |
| 39 | 2 | 3 | 5 | 00000 | 15 | 0 | 0 | 0 | |
| 40 | 0 | 1 | 1 | 0 | 16 | 1 | 3 | 4 | 0000 |
| 41 | 1 | 0 | 1 | 0 | 17 | 2 | 1 | 3 | 000 |
| 42 | 0 | 1 | 1 | 0 | 18 | 1 | 0 | 1 | 0 |
| 43 | 1 | 1 | 2 | 00 | 19 | 2 | 6 | 8 | 00000000 |
| 44 | 0 | 0 | 0 | | 20 | 2 | 0 | 2 | 00 |
| 45 | 2 | 3 | 5 | 00000 | 21 | 3 | 4 | 7 | 0000000 |
| 46 | 4 | 2 | 6 | 000000 | 22 | 1 | 2 | 3 | 000 |
| 47 | 0 | 0 | 0 | | 23 | 0 | 1 | 1 | 0 |
| 48 | 2 | 0 | 2 | 00 | 24 | 0 | 1 | 1 | 0 |
| | 57 | 60 | 117 | | | 76 | 84 | 160 | |
| Expected: | 64 | 64 | 127 | | | 64 | 64 | 127 | |

## Appendix VI

### Distribution of Quadrant Line-Totals (Hits per 125 Trials) per 60,000 Trials

| *Score* | *R* — | *H. W.* | *Expected* |
|---|---|---|---|
| 7 | 0 | 0 | .002 |
| 8 | 0 | 0 | .007 |
| 9 | 0 | 0 | .021 |
| 10 | 0 | 1 | .062 |
| 11 | 0 | 0 | .163 |
| 12 | 0 | 0 | .388 |
| 13 | 0 | 1 | .843 |
| 14 | 1 | 1 | 1.686 |
| 15 | 5 | 4 | 3.119 |
| 16 | 2 | 11 | 5.361 |
| 17 | 10 | 10 | 8.593 |
| 18 | 12 | 17 | 12.889 |
| 19 | 15 | 21 | 18.147 |
| 20 | 16 | 19 | 24.048 |
| 21 | 28 | 25 | 30.056 |
| 22 | 43 | 37 | 35.529 |
| 23 | 37 | 39 | 39.768 |
| 24 | 37 | 34 | 42.253 |
| 25 | 35 | 45 | 42.667 |
| 26 | 46 | 34 | 41.026 |
| 27 | 35 | 35 | 37.617 |
| 28 | 38 | 31 | 32.922 |
| 29 | 24 | 25 | 27.523 |
| 30 | 26 | 26 | 22.018 |
| 31 | 24 | 23 | 16.866 |
| 32 | 19 | 10 | 12.387 |
| 33 | 6 | 13 | 8.726 |
| 34 | 5 | 4 | 5.903 |
| 35 | 5 | 6 | 3.840 |
| 36 | 4 | 4 | 2.399 |
| 37 | 0 | 2 | 1.443 |
| 38 | 3 | 1 | .835 |
| 39 | 2 | 0 | .466 |
| 40 | 0 | 1 | .250 |
| 41 | 0 | 0 | .130 |
| 42 | 1 | 0 | .065 |
| 43 | 0 | 0 | .031 |
| 44 | 1 | 0 | .014 |
| 45 | 0 | 0 | .006 |
| 46 | 0 | 0 | .003 |
| 47 | 0 | 0 | .001 |
| | 480 | 480 | 480.073 |

## Appendix VII

### Distortions in Quadrant Line-Total Distributions

### (As Found in Figs. 12 and 13, and Appendix VI)

| Subject | (1) Scores of 19 and under. | (2) Scores of 30 and over. | Total of (1) & (2) |
|---|---|---|---|
| R —— (Fig. 13) | 45 (12.3% *deficiency*) | 96 (27.4% *excess*) | 141 (11.3% *excess*) |
| H. W. (Fig. 12) | 66 (28.7% *excess*) | 90 (19.4% *excess*) | 156 (23.1% *excess*) |
| Expected (Appx. VI) | 51.281 | 75.383 | 126.664 |

# A Summer Session Course in Psychical Research at Harvard University

GARDNER MURPHY

In honor of William James's centenary this year, the department of psychology at Harvard University felt that it would be appropriate to offer a course in psychical research in the summer session. The present writer was invited to conduct the course, which was entitled *Psychical Research from William James to the Present,* and offered during the six-weeks period beginning on June 29, 1942.

Six undergraduate students, three auditors, and a post-doctoral assistant in the psychology department attended. The syllabus printed below shows the ground which was covered. It was felt advisable to organize the material in such a way as to show the historical evolution of the subject, i.e., the progressive development of new interests and methods. Accordingly, the first week was given to the early study of telepathy and related matters as exemplified in the pioneer analyses of veridical dreams, apparitions, and the like. The second week was devoted to the classical studies of mediumship as illustrated in the work of Mrs. Piper. During the third week the evidence for survival offered by the cross-correspondences was studied. The fourth week surveyed the gradual development of the experimental and statistical approach to telepathy, clairvoyance, and mediumship during the present century up until 1930. The fifth week concentrated upon the Duke University studies and other studies inspired by the Duke work. The final week was given to a consideration of present research problems and the immediate issues upon which psychical research is concentrating.

The course was conducted by means of lecture and discussion centered in the readings just described. The writer, in directing discussion and in answering students' questions, attempted to clarify the relation of psychical research

to other scientific endeavors, discussed the nature of trance and of the unconscious, and evaluated the various research methods in use. Emphasis was placed upon the need for a coherent philosophical view which would define time and space in such a fashion as to permit the data to be placed in orderly relation to the data of the established sciences.

An outstanding feature of the course, from the writer's point of view, was the lively interest of the students in experimental research of their own. The piece of apparatus devised and built by Dr. Ernest Taves, known as the "KR" machine*, was exhibited during the session and four members of the group used it in research, two of them doing sustained and valuable work. One student, in fact, built in the Harvard Laboratory shop a piece of apparatus which approximately duplicates the original. Acting as experimenter, this student collected a large amount of data, and at the time of the termination of the course his major subject had achieved a significantly high scoring level. At the close of the session, arrangements were made with Professor G. W. Allport of Harvard for this student to continue his researches at Harvard along those lines upon which we had agreed.

The net impression warranted by the comments and performance of the members of the group is that the serious, arduous, and difficult work required by the course was willingly, indeed eagerly, undertaken by a sufficient number of students to justify the offering of similar courses on other occasions and at other institutions.

It may be added that a course similar to the one outlined in the syllabus below might well be given at the A. S. P. R. next winter if sufficient interest develops.

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*For a description of the "KR" machine, see "Current Plans for Investigations in Psychical Research" by the present writer and Dr. Taves, which appeared in the January, 1942 number of this JOURNAL.

# Syllabus

## Psychical Research from William James to the Present

Abbreviations: S.P.R. —Society for Psychical Research (London)
A.S.P.R.—American Society for Psychical Research
B.S.P.R.—Boston Society for Psychic Research

Week of June 29th.

### Problem: Spontaneous Telepathy

Gurney, E., Podmore, F., and Myers, F. W. H., *Phantasms of the Living,* (2 vols.); Synopsis, vol. I, pp. xi-xxxiv. Cases (telepathic impressions, veridical dreams, reciprocal and collective hallucinations, etc.):

Vol. I

14, pp. 104-6
17, pp. 188-9
20, pp. 194-5
21, pp. 196-7
23, pp. 199-202
28, pp. 210-2
29, pp. 212-4
32, pp. 218-20
34, pp. 222-5
35, pp. 225-7
36, pp. 227-9
48, pp. 246-7
59, pp. 258-60
64, pp. 264-5
65, pp. 265-7
76, p. 280
77, pp. 281-2
81, pp. 285-6
94, pp. 318-20
114, pp. 345-6
130, pp. 365-6
131, pp. 366-7
134, pp. 370-3
138, pp. 375-7
Also pp. 295-312 (The relation of dreams to the argument for telepathy.)

Vol. II

242, p. 61
296, pp. 139-41
308, pp. 164-6
309, p. 173
345, pp. 235-6
352, pp. 248-50
Also pp. 1-28 (The theory of chance coincidence) and pp. 277-316 (Note, by Mr. Myers, on a suggested mode of psychical interaction.)

*Proceedings,* S.P.R.

Vol. III, pp. 95-98
" VII, p. 32
" VIII, pp. 311-32 (Morton haunted house)

*Journal,* A.S.P.R.

Vol. XVII, pp. 82-107 (Four peculiarly characterized dreams—W. F. Prince)

*Journal,* S.P.R.

Vol. XI, p. 269
" XI, p. 278
" XII, p. 322
" XV, p. 289
" XV, p. 259
Vol. XVI, p. 51
" XVII, p. 230
" XVIII, p. 25
" XVIII, p. 51
" XIX, p. 76
Vol. XXII, p. 88
" XXIV, p. 379
" XXVI, p. 133
" XXVII, p. 326

Week of July 6th.

## Problem: Mediumship

*Proceedings,* S.P.R.

Vol. VI, pp. 651-9 (letter to F. W. H. Myers from William James about the latter's sittings with Mrs. Piper).

Vol. XIII, pp. 284-311: *A Further Record of Observations of Certain Phenomena of Trance,* Richard Hodgson.

Vol. XV, pp. 16-38: *Discussion of the Trance Phenomena of Mrs. Piper,* Mrs. Henry Sidgwick.

Vol. XXIII, pp. 2-121: *Report on Mrs. Piper's Hodgson-Control,* William James.

James, William, "What Psychical Research Has Accomplished" In: *The Will to Believe and Other Essays,* pp. 300-328.

Bergson, Henri, Presidential Address, *Proceedings,* S.P.R., Vol. XXVII, pp. 157-175.

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Week of July 13th.

## Problem: Cross-Correspondences

*Proceedings,* S.P.R.

Vol. XXI, pp. 170-191; 297-303; 369-391: *On the Automatic Writing of Mrs. Holland,* Alice Johnson.

Vol. XXIV, pp. 222-263: *Second Report on Mrs. Holland's Script,* Alice Johnson. (The "Sevens" Case)

Vol. XXIV, pp. 86-144: *Further Experiments with Mrs. Piper in* 1908, J. G. Piddington, et al. (The Lethe Case)

Vol. XXV, pp. 113-175: *Evidence of Classical Scholarship and of Cross-Correspondence in some New Automatic Writings,* Sir Oliver Lodge. (Lethe Case — Mrs. Willett)

Vol. XXIX, pp. 197-243: *The Ear of Dionysius: Further Scripts Affording Evidence of Personal Survival,* G. W. Balfour.

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Week of July 20th.

## Problem: Experimental Methods in the Study of Telepathy, Clairvoyance, and Mediumship, 1900-1930

*Annals of Psychical Science,* Oct.-Dec., 1909, pp. 561-581: *Thought Transference,* F. L. Usher and F. P. Burt.

Warcollier, R., *Experimental Telepathy,* B.S.P.R., pp. 17-73.

Coover, J. E., *Experiments in Psychical Research,* pp. 48-55; 105-108.

Bulletin V, B.S.P.R., *A Contribution to Experimental Telepathy,* G. H. Estabrooks.

*Compte-rendu Officiel du Premier Congrès International des Recherches Psychiques,* pp. 396-408 (Readers not familiar with French should substitute *The Case for and against Psychical Belief,* edited by Carl Murchison, pp. 268-70).

*Proceedings,* S.P.R., Vol. XXXI, pp. 241-269: *An Examination of Book-Tests Obtained in Sittings with Mrs. Leonard,* Mrs. Henry Sidgwick.

*Proceedings,* S.P.R., Vol. XLI, pp. 345-351: *An Experiment in "Clairvoyance" with M. Stefan Ossowiecki,* Theodore Besterman.

Thomas, J. F., *Beyond Normal Cognition,* B.S.P.R., pp. 1-14; 50-65; 70-81; 149-165. (1937).

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Week of July 27th.

## Problem: The Duke University Experiments and their Sequels

Rhine, J. B., *Extra-Sensory Perception,* B.S.P.R., pp. 126-31

*Journal of Parapsychology*

Vol. I, pp. 70-80; 102-113; 143-155; 163-171
" II, pp. 84-94; 273-286
" III, pp. 121-158
" IV, pp. 159-190
" V, pp. 1-57; 138-180

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Week of August 3rd.

## Problem: The Present-Day Situation

*Proceedings,* S.P.R., Vol. XLII, pp. 49-103: *Report on Cases of Apparent Precognition,* H. F. Saltmarsh.

*Proceedings,* S.P.R., Vol. XLVI, pp. 152-198: *Fresh Light on Card Guessing—Some New Effects,* S. G. Soal.

Pratt, J. G., et al., *Extra-Sensory Perception After Sixty Years,* 249-326.

# Picture-Tests

REV. W. S. IRVING
(Hon. Associate, S. P. R.)

## Introduction

It is indeed a privilege to be allowed to place before the readers of the JOURNAL of the American Society for Psychical Research some of the evidence that I have obtained at sittings with Mrs. Osborne Leonard. I have chosen to present my picture-tests not only because they are of greater interest to me than any other of my tests but also because, to the best of my knowledge, they are the invention of my own communicator, Dora, who purports to be my wife. I may say, in passing, that to my mind these tests are characteristic of her—that is to say, they are the sort of puzzles in which she was interested when here, and in which she would have delighted.

The picture-tests were not, I think, given to me in the first place with any idea of offering new or startling evidence for prevision; rather they were an attempt to convince me that the material that I was receiving was not taken by the medium from my own mind. Most of the pictures selected for description, therefore, have been found to contain one or more factors that are associated with my own past, or with that of my wife: something that would be of interest to both of us. In many cases these tests seem to say, "Do you remember when we did this or that?" or, "Here is a place that we visited together," etc. They are not, or perhaps it would be better to say they were not, at first, at any rate, chosen simply as good pictures to describe, but as being pictures calculated to awaken memory. It is this fact that makes these tests so impressive to me personally; unfortunately it is not easy to convey that impression of reality to the average reader, who knows nothing about my wife or myself, apart from my annotations to the sittings.

My communicator's method of selecting her picture-tests has, in the course of time, changed somewhat; at first they were very largely taken from cinema films. I was told that "almost immediately" I should see a certain picture, which was then described. I found that, generally, within the next few hours, at the cinema, something like the picture described would be seen in the programme.* Unfortunately, it was very difficult to get these pictures properly witnessed; this difficulty seems to have been grasped by my communicator, for her tests from films and other sources became less and less frequent and were replaced by the long series of tests taken from daily newspapers.

Any leakage of information to Mrs. Leonard can, I feel certain, be entirely ruled out because of the nature of the material I received. For years she knew nothing whatever about the kind of evidence that I was getting. She did not know to what daily paper I subscribed; even if she had known this, it is not easy to see how she normally could have obtained access to material which was in most cases *unpublished* at the time of the sitting. In many of these picture-tests, too, knowledge is shown of material at the sides, or on the back of the principal picture—material the position of which in the newspaper had often not been decided upon at the time of my sitting.

When the newspaper picture-tests began I was regularly taking in the *Daily Mail;* it is, therefore, from this paper that a large number of my tests appear to be taken. It frequently happened, however, that my landlady in London would also give me the *Daily Express* to read. I also often bought the *Evening News.* It is necessary to give these details in order to show the amount of material from which a choice of pictures could be made. In trying to assess the value of these newspaper picture-tests the greatest difficulty is caused by the large amount of opportunity there is for

*For other published material of Mr. Irving's, see *Proc.* S. P. R., Vol. XXXVI, pp. 190-298. Reports of cinema picture-tests appear in this volume on pp. 266-268 and 283-285. For three picture-tests similar to those presented here, see *Journal* S. P. R., Vol. XXVIII, pp. 280-285 and pp. 299-304. *Ed.*

chance coincidence. True, the number of pictures in such a paper as the *Daily Mail* is not so large as in a fully illustrated paper; there are enough, however, to leave a good deal of room for chance. Each reader will have to come to his own conclusions as to whether the correspondences between Feda's statements and the newspaper material are due to anything more than chance coincidence.* In order, if possible, to diminish chance possibilities I have from time to time attempted to limit my communicator's field of action; that is to say, I have abstained from looking at any newspaper, or any part of any newspaper, except the middle sheet of the *Daily Mail.* I have also on several occasions changed to the *Times,* a paper in which the number of illustrations is limited. These attempts, however, have not been very successful. It would seem that a fairly wide field from which the communicator can choose is necessary to the success of a picture-test. Unless a picture is definite as regards subject and has something unusual or striking about it there is little for the communicator to describe, and personal reminiscences become impossible because there are no associative links to be found in the material at hand. When I have asked for tests from a particular newspaper I have not always obtained what I wanted; my experience has been that it is best to interfere as little as possible with the communicator, even though there be left more scope for chance coincidence.

It must be remembered that we have no idea whatsoever how the pictures are *seen* by the communicator. It is obvious, however, that the "seeing" must be something quite different to what we understand by the term. More often

*In regard to the element of chance in picture-tests, see *Journal* S. P. R., Vol. XXVIII, pp. 284-285 and pp. 302-304. Mrs. W. H. Salter compared the statements made in one of Mr. Irving's tests from the *Daily Mail* with material appearing on the picture pages of 12 later issues of the same paper. On the basis of this comparison, Mrs. Salter concluded that there is "a *prima facie* case for supposing that the relation between the statements made at the sitting . . . and the illustration appearing (on the test-page) is due to something more than chance." Miss Nea Walker followed a somewhat similar procedure in evaluating another of Mr. Irving's tests. She concluded that "in no paper studied was there anything approaching to the sequence of the references which were offered as the test." *Ed.*

than not, I think, the test picture, when found, is not what we expected to find—although it fits the description given by the communicator better than chance could easily account for. Take, for instance, the description of the "person bent or bending" in the Dorothea test on page 256. It would seem reasonable to suppose that the idea of a figure bending comes from the sign "Bends" in the middle picture, yet it is not possible to tell why the presence of the word in the picture should have influenced the test in the way it did. There is, too, a somewhat similar complication in this same test in the way the word "embellished" is used in the description of what appears to be a Belisha beacon. It would seem to be beyond chance coincidence that just this word is used here, for my communicator has never used the word before in any of my sittings. Yet no one can tell the reason for the distortion. Of course, there is the possibility that Feda heard and misinterpreted what the communicator tried to give, but that is surmise only, and to attempt to discuss it would be to open up the whole problem of what mediumistic controls and communicators really are. The point I wish to make here is that, in view of our ignorance of the *modus operandi* of picture-tests, it is better to interfere as little as possible with the communicator. Otherwise good results will probably not be obtained.

And now, without further preamble, I present a few of my picture-tests, chosen for the purposes of this paper from some of my later Leonard sittings. None of them has been published before. Feda's statements are verbatim, except for some repetitions which have been omitted, and my original annotations are essentially unchanged. We understand that a picture-test may be taken from one picture only, or from one picture together with its framework—that is, the material round about the picture. Sometimes it is necessary to translate the term "framework" rather liberally; on occasion Feda's statements seem to relate rather strikingly to material which, although on the test-page, does not touch the principal test-picture. A picture-

test may also be taken from a series of pictures on one side of the page of a newspaper. We have been told, in addition, to examine the back of the picture-page, as sometimes material on the back of the page is "seen" and so becomes included in the description of the picture. Examples of the above may be found in the cases which follow:

## I PICTURE-TEST NO. 10.

*Introductory Note*: Nothing was said at this sitting about the daily paper in which the test-picture would be found. As I had mentally asked my communicator that the tests should be taken from the next day's *Daily Mail*, whenever possible, I had hoped that it would be found in that paper. This, however, was not the case. I found what I consider to be the verification of this test in the *Evening News*.* The sitting began at 10.25 A.M. on May 7th, 1935, and my communicator, through Mrs. Leonard's control, Feda, began to give this picture-test within five minutes of the commencement of the sitting. She had finished giving it by 10:50 A.M.

*Extract from a sitting with Mrs. Leonard, Tuesday, May 7, 1935.*

*Sitter*: Rev. W. S. Irving — *Recorder*: Mr. Oliver Gatty

FEDA

Now, Mr. Bill! Before she go any further, can she do the pictures? 'Cos she's anxious to do them while the power's strong, do you see? She sees one that makes her laugh, because its like a travesty—travesty—travesty. That's right! Travesty of a Spirit-picture, do you understand? It's like a travesty of a Spirit-picture and its very absurd. She thinks

ANNOTATIONS BY MR. IRVING

At about 4:45 P.M., the same evening, I bought the *Evening News* at Victoria Station. On the front page was one of Poy's cartoons, *Tales We Will Have to Tell*. "Old Sol," seated in a chair, is addressing the planets, Saturn, Mars, etc. Now, one of my wife's sisters was named "Sola." She was called "Sollie" or "Sol" by us, and was some-

*As the verification for this test appears to be found in a newspaper published on the same day as the sitting, we asked Mr. Irving to ascertain at what time the *Evening News* for May 7th could be purchased. He has sent us a letter from the assistant editor of the paper, Mr. V. Suhr, who says that "the first edition of the *Evening News* of May 7, 1935 to contain the Poy Cartoon was printed by 3:41 P.M. on the day in question and was probably on sale near the office by 4 P.M." It should be noted, therefore, that the test was completed more than four hours before the edition containing the cartoon came off the press. *Ed.*

**TALES · WE WILL HAVE TO TELL**

you will laugh too, but there is something in it that looks like a ghost, or a Spirit-form. She kept getting the feeling it's meant to be a Spirit-form, or it looks like

times alluded to as "Old Sol."* She died eight years ago. If this be taken as a Spirit-picture of her, it is indeed a "travesty." Sola was slight in build. In addi-

*In connection with this point, Mr. Irving has kindly sent us a statement from his sister-in-law, Mrs. Snell. Mrs. Snell says that her sister Sola was frequently referred to in the family as "Sol" and that it was their habit to use the expression "old" to denote anyone of whom they were speaking in an affectionate manner. *Ed.*

a Spirit-form. It's coupled with something that is so absurd, you see, absurd!

tion to the light rays proceeding from Old Sol's head, perhaps suggesting a halo, there are the words, "It was simply glorious. The greatest thing ever. I know, children, because I was there—in the very front row!" The words refer to the Jubilee procession.

She also felt L. S. D. Something representing money, or L. S. D. Got a feeling of money in a very definitely crude sense, definitely crude sense. That's right! (Yes?)

The idea of money is not present in the picture, unless one takes the word "Silver" out of its connotation. I think it possible, however, that the letters L. S. D. are derived from the letters in **OLD SoL**, Feda then falsely elaborating on this impression. Dora has never before used the letters L. S. D. (pounds, shillings, and pence) in referring to money.

Oh! What do you think it is supposed to be, Dora? 'Tisn't puddings, is it? What are they? Round? Are they faces or puddings? Well! She keeps drawing some round circles—rather fat circles, and yet there are markings—marks in the circles, and streaks—little bits sticking out of them. They looks like puddings, tied up, you know, Mr. Bill. (Yes?) Not a pudding in a basin, but a fat pudding turned up all round. Most people put them in basins, then they wouldn't be round. This is quite round. I'm not sure if it is a pudding, it might be a face 'cos lots of Englishers got faces like that. I see them and feel they're important in the picture. Dora says, 'Yes, they are. They're important in the picture.'

The faces of Old Sol and the Planet children resemble puddings. The effect is enhanced by neckties tied tightly under their chins. It is not surprising that Feda was at first puzzled as to whether faces or puddings were being shown. It is correct, too, that there are "bits sticking out." Neptune holds his trident, Saturn has a double ring round his globe-shaped face, and Mars wears a helmet.

Something representing a trail. Something laid like a trail, or a connecting line—a connecting line, Mr. Bill. Laid like a trail or a connecting line. That's right, Mr. Bill!

There is a thick and wavy black line which crosses the picture, passing behind the chair of Old Sol and behind the planets.

Anything else, Dora? Mr. Bill! Look! She only showed me one of these round things I told you about, but she seemed to say there should be more of them, but *one* important one. Showing me more than one. That's right! Mr. Bill? (Yes?) Mr. Bill! Wait a bit! She doesn't want to give a lot of things, you see, out of a picture. She want to give a few that is very clear. (A lot are rather important, Feda.) Yes, but what she want to get is the main features, and she'd rather be clear about six than not quite clear, only suggestion, of a dozen.

The important one of "these round things" is, of course, Old Sol. The description of being "like a pudding" applies equally well to him as it does to the smaller figures. It is interesting that the number *six* is mentioned here, as there are six planet children shown in the picture.

Mr. Bill! She couldn't see this in the picture, but she got a suggestion about time, as if there might be something about clocks or almanacs, something registering time in the picture—an indication of time or the passing of time, do you see?

The presence of the sun in the picture might be considered as suggestive of "time" and "almanacs." The words SILVER JUBILEE definitely indicate the *passing* of time.

It's what Dora call an untidy, unkempt picture. A suggestion of unkempt—unkempt—unkempt, untidiness about it. Dora didn't like that, Mr. Bill. She liked things to be kept straight and right, tidy. She was very good on the earth at making plans for things, making it easy to be tidy. She rather clever at that, and making it easy for other people

It is, I think, the rays of the sun, uneven in length, which give to this picture the "untidy" appearance to which Feda refers. And Dora may well have been struck by the fact that two of the little planets are sitting down in the presence of their instructor, while the rest are standing. The points of character given here concerning my wife appear to me

too. Do you remember, she used to help to keep people in order at one time, Mr. Bill? What did she do? 'Push them,' she says. 'You go there. You stand there. You come along.' As if she was touching their arms sometimes, and I feel they would take notice of her. More than of Mr. Bill? Yes! You don't mind my saying so? They did. She got a gift of doing things that way. She wouldn't get excited and shout. You would be more excited than she if you were doing the same thing. You wouldn't shout—you'd shiver and shake! You wouldn't keep as still as Dora. You remember how still Dora could be? She knows the word Feda want—'repose.' Dora had got that, do you see? But she could laugh and be active. She wasn't heavy—no! She was bright and light, full of energy, but she could be quiet and people would obey her.

Monkeys? Was there any people who behaved like monkeys, Dora had to see to? Not real monkeys — people behaving like monkeys she had to look after. Do you remember someone called Lun—Lunt—Lund—Lunt connected with that? Lunt. A name beginning with L. Sound like Long—Lunt. No! 'Don't think now, put it down, put it down,' she says. This all reminds her of happy times, Mr. Bill, talking about this, very happy times. And a photograph. You may have forgotten, Mr. Bill, but a photograph reminds her of what she's been talking about, a photograph to be excellent, especially with regard to "repose."

The people who "behaved like monkeys" I take to be the children that my wife used to teach. The test-picture would certainly remind her of her teaching days. I was very well acquainted with a family named Long who left Newent (where I now live) to live in Leckhampton. About ten years ago I revisited my old parish at Leckhampton and called upon the Longs. My wife never knew the Longs, for they moved to Newent years after her death, but a relation of theirs was here during her life-time. In my Leonard sittings an attempt was made to refer to Leckhampton as

of her being taken with the monkeys. Yes, there was! Not bad; not flattering, but not bad. Dora's at one side, not middle of picture, one side—I think at left. On the left, facing the picture, Mr. Bill! When she give it, I got to give it again, but I keep on repeating it. I think when Dora builds it up in these conditions she'd lose it. That all came out of the picture in a way. Talking about these times was all suggested by the picture—that's how she introduced it—wait a bit—by making Feda say, 'I didn't like things to be untidy. I made Feda introduce it.'

A picture of balloons? God save the King! [Feda sings.] Something either on the edge of the picture, or above or below the picture—something about balloons. Not a song about them—that one people sang yesterday. God save gracious balloons. I was only singing that to keep her quiet.

She gets Australia—Australia's very near to the picture, but not quite connected with it—very very close. And Spain. Spain or rain? Spain. Australia and Spain. Now, have you got that, Mr. Bill? (Yes.) Sure that's all right . . .

far back as 1923 (See *Proc.* S. P. R., Vol. XXXVI, pp. 255-263). There, too, was confusion, but Dora got as far as "Lek," "Leckton," and "Leckan." The photograph that Feda refers to may be one that I still have of my wife, seated among the eleven girls that formed her Bible Class at Leckhampton. This photograph was definitely referred to at a Leonard sitting in January, 1935, when it was also stated that there "was more than one taken." I have only one in my possession. An enlarged framed copy of this photograph was presented to my wife at the time when it was taken. It now hangs in my bedroom. There may be some confusion here between the photograph and the test-picture. In the photograph my wife is seated in the midst of her group of pupils; in the test-picture Old Sol is on "the left, facing the picture."

The words concerning the Jubilee that are put into the mouth of Old Sol are enclosed in an oblong-shaped balloon, just below the top of the picture.

Although the word *Australia* appears on the test-page, it is not "very very close" to the picture. It is about 15 inches away, in the left lower corner of the page. I cannot find the word Spain anywhere on the test-page.

*Extract from the Personal Control that followed*:

[Dora]: I do hope that picture-test will be a good one. I want it to be a good one today. Do you remember, I called you something sometimes beginning with S? A special name beginning with S.

I remember no name beginning with S by which my wife called me. The mention, however, of the letter S is interesting. The central figure in the test-picture is, of course, named Sol.

## II PICTURE-TEST NO. 14.

*Introductory Note*: Nothing was said at this sitting about the daily paper in which the test-picture would be found, nor had I made any requests to my communicator about this matter. The sitting started at 10:25 A.M. and Feda began to give the test at about 11 o'clock.

*Extract from a sitting with Mrs. Leonard, Thursday, September* 19, 1935.

*Sitter*: Rev. W. S. Irving — *Recorder*: Mr. Oliver Gatty

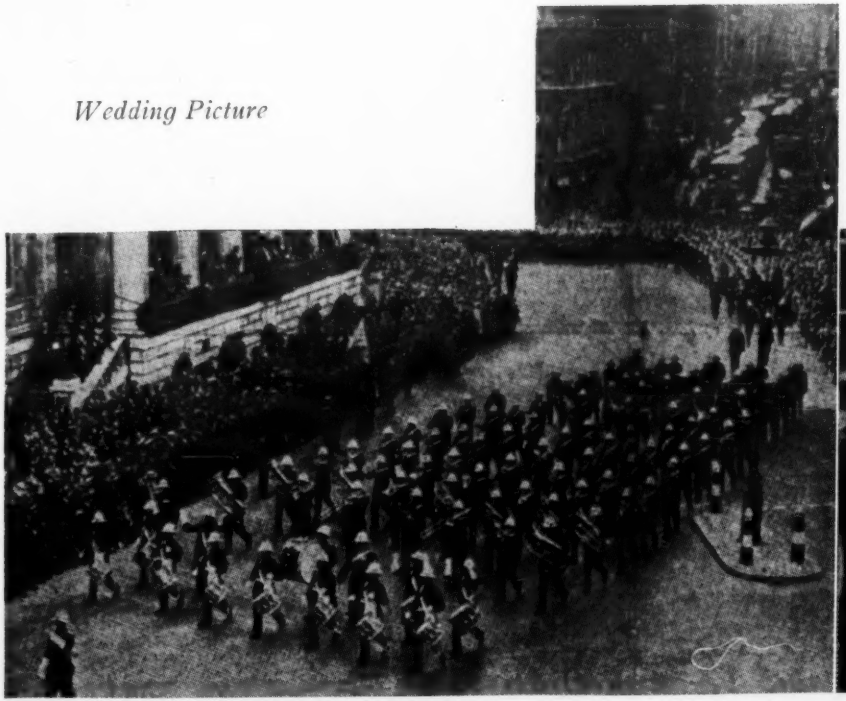
FEDA

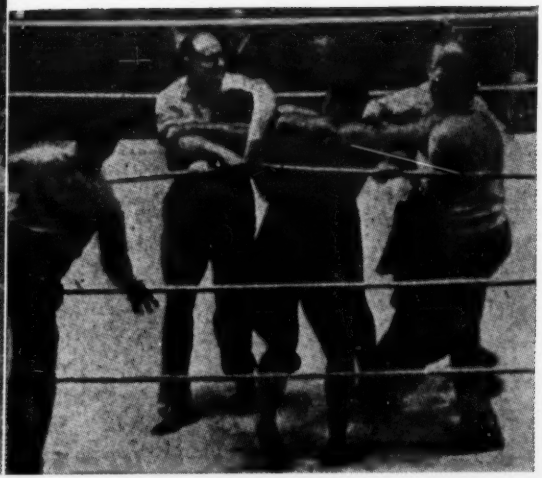
Will you do the picture next, Dora? Yes! She'll do the picture next! Something very funny came to her mind in viewing this picture mentally. She felt as if she'd summed something up in this picture. Apart from any detail, she felt she was summing it up and that it amounted to—she's laughing! Pulling up one's socks—pulling up one's socks. That is a sort of description, or summing up. She could write under it, that would be applicable. Is that right, Mr. Bill? (Yes.) Ap-plicable.

ANNOTATIONS BY MR. IRVING

On the back sheet of the *Daily Express* for Friday, September 20, 1935, the day after the sitting, the pictures described below were found. Almost in the center of the page was a picture approximately 6″ by 5″ entitled, "Royal Marines March Through City." The words under the picture continue, "Two-centuries-old privilege was exercised for the first time yesterday by the Royal Marines. Battalion which has mounted guard at royal palaces marched through City . . ." The band of the battalion is shown in the foreground of the picture, at the head of the battalion. In my opinion there is nothing more bracing than the strains of martial music. I know that my wife thought so, in life.

*Wedding Picture*





There's a drinking vessel in this picture — just something to drink out of. I must call it that because it's not quite—this is very important! It's not quite an ordinary drinking vessel—this is the most important part of it! It's something unusual, yet, in the picture, it is suggested as a thing out of which one *must* drink, or *should* drink. Put it this way—not *must,* please, one *should* drink. You will see exactly why I put it in these words.

In the left upper corner of the page is a picture, approximately 8″ by 7″, headed "Breeze Veiled the Bridegroom, Too." The picture is of two people, who have just been married. They are standing at the door of St. Paul's Church, Knightsbridge. There is no drinking vessel in the picture, but I take it that the test is symbolical. The bride and groom hold in their hands the cup of happiness, out of which they should drink, but out of which there is no compulsion for them to drink.

Right-hand side, right-hand side of the picture, I got a feeling of using strength—using strength, like a test of strength—something to test one's strength—a test of strength.

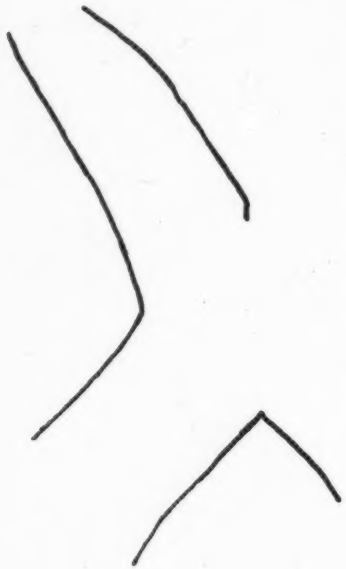
A considerable section of the *right-hand* side of the page is taken up by a series of pictures, one above the other, of a wrestling match. The heading reads, "Really All-In Wrestling." Below the series of four pictures are the words, "Struggle began in earnest when bout officially ended in Australian wrestling match between Tom Lurich and Tiger Duala. Referee announced result was a draw. These pictures from British Movietone News reel show what followed. Wrestlers refused to stop. Referee and seconds tried to separate them—and 'all-in' took on a different meaning."

What's that, Dora? Is it a lamp post? See thing, a shape in the picture looking rather like a lamp post. May be something else, but it's that shape — like a lamp post.

The Band of the Royal Marines, in the picture in the middle of the page, is shown passing an island in the road. In the center of the island is a lamp post. This lamp post is tall and fairly conspicuous.

Seems to be a kind of signboard in it. (In it?) In the picture —not in the lamp. Signboard. Signboard.

What's that, Dora? Oh! I'll have to draw this, Mr. Bill! [Sitter holds his notebook open and Feda draws in it. A tracing of this drawing is reproduced below.]



Yes, like that! Can you see it, Mr. Bill, because I'd better draw it again if you can't? Funny shaped thing, like—there are funny shaped lines in the picture. I'm very good at drawing, aren't I, Mr. Bill? I use a sense of the medium's for that. She would have been very good at drawing. The medium would have been very good at it if she had learnt.

On the island in the roadway, mentioned above, are three posts set at angles round the lamp post. They are painted black and white.

On comparing Feda's drawing with the picture of the Royal Marines, it will be seen that the outlines correspond quite closely. The left-hand curved line follows the line of spectators outside the Mansion House, the angle at the bottom of the drawing, to the right, being formed by the island, and on the left-hand side by the members of the band as they march down the road.

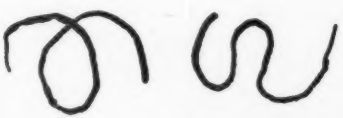
Are you ready, Mr. Bill?' (Yes.) Er—in the picture, or practically touching the picture, very very close to it, see if you can see a name, looks to her like a name: B-R-E-S, B-R-E-S, B-R-E-S (Yes?)

A quarter of an inch above the picture of the bride and groom, in the left-hand upper corner of the page, is the word: BREEZE. The word is in large type, ¼" in height and ⅛" in length.

There's something to do with a corner. This comes back in the picture. BRES, she felt near it, not quite in it. This is a corner. In the picture there is a corner. A corner is depicted—depicted. Is that right, Mr. Bill? Now, I may be getting this idea of a corner in a symbolical sense — I may! I'll have to leave that to your judgment. That is a difficulty I find myself in sometimes with regard to the book-tests and with the pictures. That right, Mr. Bill? (Yes.) Wait a minute!

The lower left-hand corner of the Wedding picture has been cut out in order to provide room for words describing the scene. Among the words are the following, ". . . and the *breeze* which played this trick on them . . ." The word *breeze* therefore occurs twice, first in capital letters above the Wedding picture, and second in ordinary type in what should have been a part of the picture itself. The right-hand corner of the Wedding picture cuts into the Royal Marines picture. This can be seen in the reproduction.

Worms? Isn't any worms in the picture, are they? Doesn't look like worms in the picture! Something about worms in the picture, sort of like this shape, do you see? Like this shape! (You draw it, Feda.) [Feda draws in my notebook while I hold it steady for her.]



Things like that! There is more than one worm, she thinks, you

The band of the Royal Marines is headed by two rows of drummers; in the first row are seven with side-drums. The drums are dark and the lacing of the strings shows up white on the dark background. There is, however, something else that is rather strongly suggested by Feda's drawing. In the right-hand top corner of the page is a picture of Sir Malcolm Campbell being congratulated on his safe return to England. Sir Malcolm is wearing a dark tie with an unusual design of white wavy lines on it. The design of the tie is very conspicuous in

see? Is that right, Mr. Bill? (Yes.) Well! That's all for the picture . . .

the picture, and the white lines are similar in outline to Feda's drawing.*

## III PICTURE-TEST NO. 15.

*Introductory Note*: November 14th, the date of this sitting, was polling-day. The next day's papers were for the most part filled with Election results and it should therefore be noted that my communicator's choice of pictures must have been extremely limited. The sitting started at 10:25 A.M. and Feda began to give the test about a quarter of an hour later.

*Extract from a sitting with Mrs. Leonard, Thursday, November* 14, 1935.

*Sitter*: Rev. W. S. Irving

*Recorder*: Mr. Oliver Gatty

FEDA

. . . Now, this picture that she gets very strongly, one of the main points [Here a voice said 'Features'] or features in it is a comparison in — what?— stature, stature! A very great comparison — a very great difference, a very strong difference.

Spreading chestnut tree! Boy stood on burning deck! She says, 'No! Nothing to do with boy stood on burning deck.' But I know English poetry! There's something in this picture reminds her of the old poem, Spreading Chestnut Tree. The boy *did* stand on the burning deck! He did! He couldn't help it. Dora doesn't know that one. She keeps shaking her head at me. She knows about the chestnut tree and she

ANNOTATIONS BY MR. IRVING

In the *Daily Mail* for the next day, Friday, November 15, middle page, to the right of the Leader, was a Poy cartoon entitled "Friday." It will be seen from the reproduction of the cartoon that there is indeed a "very strong difference" in the "stature" of the two "figures."

The large figure, General Election, has hooked a fish and is, apparently, about to hurl it out of the water onto the bank of the river by dint of his sheer muscular strength. With both his big hands he grasps his fishing rod, which is bent almost double by the weight of the heavy fish he has hooked. The two points from Longfellow's poem which may be thought to be applicable are found in the following lines:

*It was not possible to reproduce here the picture of Sir Malcolm Campbell, but the similarity between the design on his necktie and Feda's drawing seemed to us very striking. *Ed.*

**FRIDAY!**—*By POY*

What have we got for breakfast.

says, 'Let's concentrate on the chestnut tree because there's something in the picture that will remind you of the poem.' The poem seems awfully much in this picture; there's more than one point, in the picture, that's touched on. There are two at least of the main points in the poem. (Do you mean, "The boy stood on the burning deck," Feda?) No, she says. The boy isn't in this. He's got nothing to do with this. Dora says, 'Keep the boy to yourself.' They asked about him. I didn't want to say anything about him.

Now! What's that on the ground, Dora? There's something on, what you call, the ground in the picture that looks to her like a bag — a bag or receptacle. A receptacle for stuff-

". . . a mighty man is he,
With large and sinewy hands . . .
". . . the muscles of his brawny arms,
Are strong as iron bands . . ."

It is not difficult to see why Feda may have been reminded of the "boy stood on the burning deck." The bank of the river is not unlike the deck of a ship, with the water below. John Citizen looks like a small boy and, some two yards behind him, a fire is burning on the bank.

In the foreground, John Citizen is holding out a large landing net, preparatory to catching the fish. General Election has a basket attached to his belt; it is of the type that fishermen use to put

ing things into. An impression of stuffing. She thinks there must be something in this picture conveying the idea of putting or stuffing something into this thing.

What? A passage of something? Something passing through the air? In this picture there's something she felt, or thought, or got — but didn't get quite so clearly as the other things: the passage of something through the air, as if someone had thrown something through the air. There's an indication of that. An impression of what? Monstrosity? No, no! Haven't got that quite right. An impression of [Here a voice said 'Velocity'] velocity? Yes! That's right. Not monstrosity, velocity! I can always tell if you are wrong! That's what gave her, she says, the idea of something passing through the air.

Not she think, in the picture, but close to the picture, there's a reference to very old dates. The dates are given in numbers, as if, like, a long time ago given in numbers. Don't know why she said it, so I've got to give it that way. That's right, Mr. Bill.

One more little thing. It reminds her of something she once gave you before . . . See if there is something china, china, in connection with the picture itself. She doesn't know if it's the country China or something made of china, see? That's all for the picture.

their catch into. In view of the size of the fish, it would certainly have to be "stuffed" into this receptacle.

The greater part of the fish is just clear of the water and, as mentioned above, General Election is just about to hurl it through the air onto the bank. We might, therefore, take it that he is in the act of thus hurling it. I think it will be agreed that the cartoonist has drawn the picture in such a way that one has the impression of "velocity" in connection with the fish which is just leaving the water.

About 8¾" from the left-hand top corner of the picture appears in black type, "1066 and All That." This is the title of a play running at the Strand Theater; although it is an advertisement and therefore appears daily, it helps, evidentially, in fixing the location of the test-page.

There is no mention of "china" or "China" in the picture. [Mr. Irving has pointed out to us in correspondence that an association may exist between the reference to "China" and a conspicuous feature in the test-picture. He has sent us full details and we are inclined to agree with his interpretation. For personal reasons, however, it is not possible to include this annotation. *Ed.*]

## IV PICTURE-TEST NO. 24.

*Introductory Note*: Previous to this sitting, I mentally asked my communicator to try to take her pictures from the middle sheet of the *Daily Mail*. (The middle sheet has four pages from which to choose.) I notified Miss Newton (Secretary of the S.P.R.) to this effect when sending her the carbon copy of my notes on the sitting. I sent her these by registered post from Whitstable (Mrs. Leonard's railroad station), same day as the sitting, at approximately 2:20 P.M. The sitting started at 10:27 A.M.

*Extract from a sitting with Mrs. Leonard, Tuesday, April 27, 1937.*

*Sitter*: Rev. W. S. Irving, taking his own notes in duplicate.

FEDA

Now, Mr. Bill, are you ready for the picture? (Yes.) Well! In this picture there seems to have been a kind of accident—a physical accident. Someone seems to have been hurt in it. Now, Dora also got the idea of someone or something being flattened out —flattened. (Yes?) She doesn't want to put two and two together and say that would be the result of the accident—she doesn't! In her mind there seemed to be some separation or division—separation or division between the flattening-out part and the actual accident that she first noticed.

ANNOTATIONS BY MR. IRVING

It is important to note that Poy's Cartoons are at this date no longer on the middle sheet of the *Daily Mail*. In that paper, for Wednesday, April 28, 1937, the day after the sitting, the middle sheet contained the pages numbered from 9 through 12. On page 9, just below the middle of the page and in the center, was a picture (approximately 9″ by 2″) entitled, "Eight Injured In Road Collision." Beneath the picture are the words, "The ambulance at the scene of an accident . . . yesterday, when eight people were injured in a car and bus collision. The car, which was overturned, is on the left." The ambulance is shown in the center of the picture, the bus on the right, and the overturned car, which does look "flattened out," on the left. A space of six inches separates the bus from the car, which may account for the impression of "separation or division."

She felt as if somebody in this picture had got a head—like yours —out of all proportion to the body; a head that seemed all wrong for the body, didn't fit it —wasn't in proportion—it wasn't in keeping.

To the right of the accident picture, and touching it, is a picture of two people with abnormally large heads. The picture is approximately 5″ by 5″ and is an advertisement. The heads of the man and woman are globular and 1⅛″ in diameter. Their bodies are slim.

Also she got the feeling of hurrying, scurrying, running, rushing, racing. Have you got that? (Yes.)

Touching the accident picture, immediately above the overturned car, are the following lines, "They went by in a flash and a roar, cutting their way round the curves of the river, skimming under City bridges, . . . " Immediately below the car the words continue, ". . . speeding through the pool, four naval torpedo boats racing in their wake . . . "

Will you also look and see if someone is wearing an apron or protective garment? I did not see an apron, but I felt that I might describe it as an apron. Dora's laughing and she says, 'You see, my thoughts ran to apron, or overall, as a protective garment.' She says, 'You remember mine?' [Here Dora spoke at length about an apron that had belonged to her.] . . . Well! Go on with the picture, she says. She's sure she's right about that!

No one visible in the picture is wearing an apron or overall, but 1¾″ below the accident picture appear the words, " . . . donned a diver's suit yesterday . . ." A diver's suit is certainly a "protective garment."

She felt as if she wanted to say, 'A hunting we will go—a hunting we will go,' like that! There is something in the picture, she say, conveying the idea of hunting, something used in hunt-

My wife and I were fond of hunting; in two Curacies, foxhounds were kennelled in the Parish and we spent many days hunting on foot with the hounds. 3⅛″ below the accident picture

*EIGHT INJURED IN ROAD COLLISION*

*The picture of the couple with the enlarged heads was shown in the newspaper directly to the right of the picture of the accident.*

ing—something definite she says, 'Ta, ta, ta.' [Feda imitates the sound of a hunting horn.] Don't know if it's important. She wants me to do it!

Seem to be the front or outside of a building showing in this picture—only part. It doesn't show all the building, its incomplete. She says, 'I don't know what's happened to part of it; it seems to be sliced off—cut off, rather. I want to put it—ragged fashion.'

Something—wait a minute, Dora, show me that again! Yes! There's some steps, some steps showing too, Mr. Bill. Something to do with stepping up—putting your foot on steps—steps showing in the picture. The steps seem to have some special significance.

In this picture you'll see something; it says—the time has come, the hour has come, the hour has struck—that kind of thing! *Now is the time*—put that down too! Oh, that is strong in this picture, Mr. Bill, very very strong! Is you right, Mr. Bill? (Yes.) She leaves the picture there, but she think she got some strong things in it, do you see? She jumping from that now, Mr. Bill . . .

and 1¼" below "diver's suit" I found the word "Foxcroft."

At the top of the test-page there is a picture, 5" by 4½", entitled "The Bridesmaids Walk." In the background is a house, or row of connected houses—one cannot be sure which, but probably the latter. All the building does not show; it is cut off by the camera, but not, I should say, in "ragged fashion."

In the picture mentioned above, two of the bridesmaids are shown in the act of mounting a step. The step leads to All Souls' Church, Langham Place, and therefore has significance as the bridesmaids are going there for the wedding.

At the back of the test-page (that is to say, on page 10) is an article entitled "The Time To Buy." This title is in large black type. It says in the article, " . . . With such a situation as now exists, the wise investor will buy . . . The general conditions are thus exceedingly favorable for the buyer . . . in fact, now is the time to buy and secure at bargain figures the pick that the market has to offer." The words *"Now is the time* to buy" are partly at the back of the accident picture and partly at the back of the picture of the man and woman with abnormally large heads.

*Note*: It will be noticed that the "points" given in paragraphs 1, 2, 4, 5, and 8 are all close together; so are the points given in paragraphs 6 and 7. But the point in paragraph 3 is above and below the overturned car ("something flattened"), said to be "separated" from the rest of the accident picture. This is, of course, assuming that my interpretation is correct.

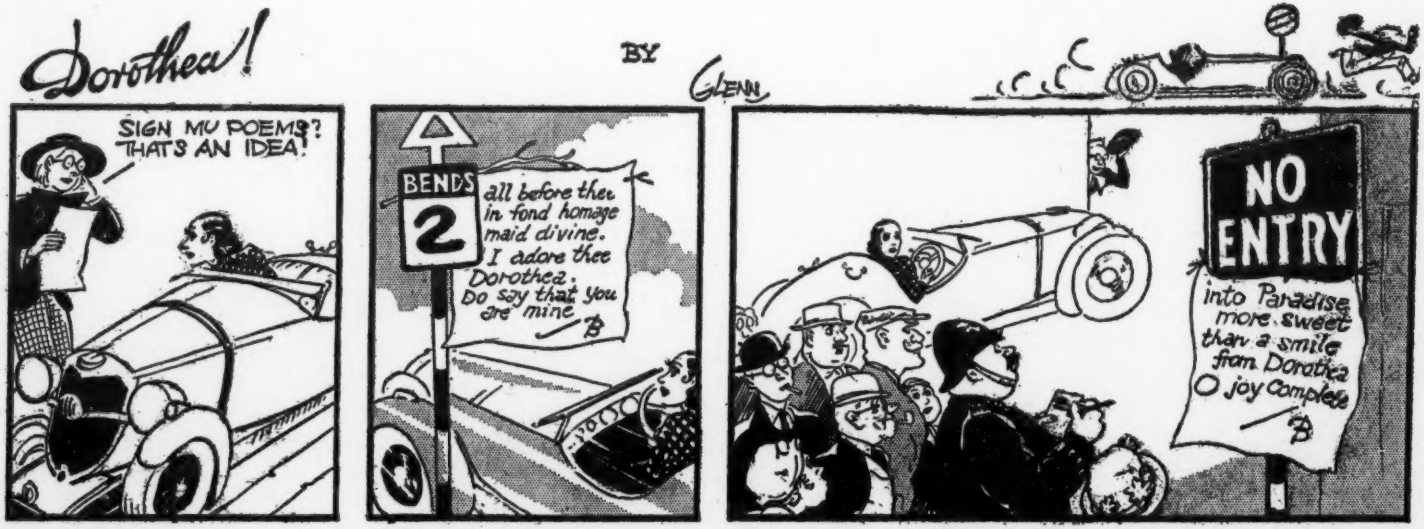
## V PICTURE-TEST NO. 26.

*Introductory Note*: Apart from saying, mentally, at home, that I hoped the picture-test would be taken from the *Daily Mail,* I did not try to limit my communicator, this time, to any particular part of the paper. When sending the carbon copy of the script from Whitstable by registered post to Miss Newton, at about 2:25 P.M. on the same day as the sitting, I wrote on the copy that the test should be found in the *Daily Mail* for Friday, July 23, 1937. The sitting began at 10:23 A.M.

*Extract from a sitting with Mrs. Leonard, Thursday, July 22, 1937.*

*Sitter*: Rev. W. S. Irving, taking his own notes in duplicate.

| FEDA | ANNOTATIONS BY MR. IRVING |
|---|---|
| Now, Mr. Bill! She want to go to a picture. (Splendid.) Wait a minute! She sees a picture of a person—a person. She can't tell yet what sex, but a person bent—bent, or bending. At first she thought it was a deformed person, and if you held the paper some little distance from you, you might think so too. 'Paper,' she said, Mr. Bill, as if not picture in a frame, but in paper. The person's full face is not towards one. Very little of the face is wisible (*sic*). [Here a voice said, "visible."] The person seemed to be holding something in the hand —in one hand. Em—the object is not very clear, but it seems to be somewhat bigger than the hand what's holding it. Wait, Dora! | On page 18 of the *Daily Mail* for Friday, July 23, 1937—the day after this sitting, there were three of Glenn's cartoons of "Dorothea." The cartoons are side by side. In the middle one Dorothea, who is seated in her car, has stopped to look at a signpost. On this post is the word BENDS, with a serpent-like line below. Dorothea's face is shown in profile, and in her left hand she grasps the steering wheel of her car. It is not very clear as only half of the wheel can be seen, but it is, of course, a good deal bigger than the hand which holds it and it extends beyond the hand. It is, perhaps, worth noting that there is reference to "paper" in Feda's remarks. Poems on paper |


Dorothea!
BY GLENN
SIGN MU POEMS?
THAT'S AN IDEA!
BENDS
2
all before thee
in fond homage
maid divine.
I adore thee
Dorothea.
Do say that you
are mine.
B
NO
ENTRY
into Paradise
more sweet
than a smile
from Dorothea
O joy complete
B

May I say that it extends beyond, and outside the grasp of the hand. She says, 'Do you understand that? It extends beyond and outside the grasp of the hand.' Please, Dora! Wait a bit! It's not something they could hold in the hand and wouldn't show—it's too big for that.

The person has on a cloak, or clothes of some description that seemed to stick out—to extend away from the body—not close or clinging clothes.

There seemed to be a seat—a seat prominently showing in this picture—a seat. I'm not sure if it's a chair or a sofa, but something a little wide for a chair.

I also see what looks like a doorway, a doorway, and I feel that the doorway is rather near the seat.

There is more than one person in this picture, and I get a very strong feeling of a difference or comparison in heights and sizes of people. As if you might say, What a little person that one is, what a big person the other one is! Do you see, Mr. Bill? It's a comparison in sizes.

Now let us go back for a moment to the person I first described. I said, at first sight, it looked like someone deformed. It does, it does look like that! But I don't think it is. An—it's

are a prominent feature in all three cartoons.

Dorothea is wearing a dark dress spotted with white. So far as can be seen, it is not close clinging. In the left-hand picture it sticks up to some extent above her neck.

In all three pictures Dorothea is shown seated in her car. The seat appears to be wide enough for two people, but she is alone in the car.

In the right-hand picture, outside what appears to be a doorway, is a notice, NO ENTRY. It is in large type, letters ⅜" in depth. Below this is a sheet of paper on which is written a poem for Dorothea.

In the right-hand picture there are eleven people shown. A large policeman and a small boy are in the group; this may account for "a difference or comparison in heights and sizes of people."

This is a return to the first item Feda gave. The source of the thought about deformity may well be the word BENDS. But a poet has been making love to Dorothea in one or two previous

important! Yet there is *about* this picture an air—a suggestion of something curtailed, limited, distorted. Shall I say, abnormal, abnormal, which may have given me the idea of deformity. (Yes?)

I dunno if this is a picture of a library, but I get a feeling this picture suggests reading, reading and writing. I feel as if a person in the picture had just been reading and writing! (Reading and writing, Feda?) Yes! That's right! Wait a bit! Reading and writing! Wait a minute!

Cajoling? Cajoling? Do you know what cajoling is? (Yes.) Cajoling. One person she feels is cajoling another to do something that I'm not sure they would be very willing to do.

In the upper part of the picture I see something like a large ornament, rather round in shape. It's light—it's light, not dark—light, and somewhat ornate, or is intended to be ornate—*embellished*.

cartoons of this long series. Dorothea has not been impressed. She has evidently told the poet to sign his name to his effusions for in the left-hand picture, today, he says, "Sign my poems? That's an idea." Wherever Dorothea goes she finds poems about herself tied to signposts, etc. Dorothea has limited the poet's love-making, and he tries these abnormal methods to attract her attention.

In the left-hand picture the poet holds in his hand a poem which he has apparently been reading to Dorothea. In the middle picture Dorothea is reading a poem the poet has written and tied to a post. In the right-hand picture a policeman is reading another of these poems while Dorothea looks on.

"Cajoling" fits the situation perfectly. The question arises, however, as to whether the word could be anticipated as likely to be appropriate through reading previous cartoons about Dorothea. I do not think that this is so. I am forwarding to the *S. P. R.* the six succeeding numbers of the Dorothea cartoons in the *Daily Mail* from July 24th through the 31st to show that the word "cajoling" would not have applied to any of these. There was no cartoon on July 30th.

Above the right-hand picture is a sketch of Dorothea driving her car. In front, or by the side of the car—one cannot tell which—is a light-colored circle on a pole. It looks, so far as one can tell

Isn't she using funny words! Embellished? Is that right, Mr. Bill? (Yes.) Embellished. Wait a minute!

from the picture, as if it were meant to be a Belisha beacon. It could, however, be some sort of an automobile mascot. That Dora interpreted it as a Belisha beacon may, perhaps, be inferred by the emphasis given to the word "embellished," misinterpreted by Feda.

Now she doesn't feel this is in the picture, but very close, or at the back, mention of horses—something to do with horses.

It is curious that an apparent reference to Belisha should be followed by a reference to horses (Hore-Belisha). Three and a half inches below the right-hand test-picture, but on the *back* of the page, I found the following; . . . "though a man can be lifted into a saddle he cannot long retain his seat without competence."

And near the horses there's the mention of a place Dora felt you and she had been to together. I feel I want to jerk it out while I can get it! Near the horses, mention of a place you and Dora had been to together. Talking about horses. Horses. [Here a voice said, 'Race-course.'] Lacy horse. [Voice again, 'Race-course.'] Do you remember place where there was a race-course, a place you and she knew when she was here, not where you are now—a race-course and you and Dora had to notice it? You were near it and had to notice it. And, one time, wasn't there something actually going on there? She says, 'We remarked on the people.' It wasn't just looking at one, like, might be anywhere, but there was something going on there. That just reminded her of it, do you see, Mr. Bill? And

Also, at the back of the test-page, 9½" below the sentence quoted above, I found the following: " . . . like a race-horse . . ." I found the word "Scarborough" in two places, at a distance of three inches and four inches from the word "race-horse." For many years my wife and I spent part of our summer holidays at Scarborough and part of them at my father's Vicarage near Richmond, Yorkshire. The grandstand of Richmond race-course could be clearly seen from the windows and the garden of the latter place. My wife died in 1918 and my father in 1929, so the details Feda gives fit well. Before leaving home I had noticed in the *Radio Times* that programmes from Scarborough were being broadcast during this week, but it is not easy to see how this knowledge could have influenced the

is there something connected with what you were doing there near this race-course? Weren't you and Dora seeing somebody else just then, who's now passed over also? You were! Don't guess, Mr. Bill! Wait a minute! The person who passed over, passed over after Dora—some considerable time after. Dora, wait!

Mr. Bill! Something to do with a prize, too, that she gets. Not in the picture, but, like, through the picture she gets the idea, prize, prize. I've got a prize. I've won something in rather a big way! Oh! Dora's laughing! She's reminded about something she did get by way of a prize when she was here on earth, and she's laughing, and she says, 'It wasn't very valuable.' But you and she laughed about it. It was a very happy time, a very happy time, and the prize wasn't valuable, but it so amused you that she'd got it. She says, 'Do you remember trying for the prizes?' Something you and she did together used to amuse you and interest you, trying for the prizes! I don't think she particularly wanted this prize she did get! I think she'd rather have had something else, do you see? You've either got the thing at home—the prize—or something that reminds you of it. Sure it is! Sure it is! You know, she gives me very strongly the feeling, 'I wish it had been so and so instead.' But I'm sure you've got the thing, Mr. Bill . . .

tests; it is by no means the general rule for radio programmes to appear at the back of the Dorothea cartoons, nor does the advertisement which contained "race-horse" usually appear there. My wife and I once followed the hounds on Scarborough race-course, and it was at Scarborough that we last met my wife's sister, Sollie, before my wife's death. Sollie died later.

1⅞" below the right-hand test-picture, but, as Feda states, "through the picture," that is to say, on the back of the page, I found the following: " . . . It hands out no easy prizes . . . but the prizes are there, just as they have always been, for those who have the guts to earn them . . . " When my wife and I were living in Gloucester in 1912-13 we used to go in for prize competitions in the hope of winning some valuable monetary prizes. The only prize that my wife won, however, was a bound copy of *The Grand Magazine*, July to October, 1912. We would, of course, have preferred a cash prize. This volume is still at the back of my desk, and I am frequently reminded of the incident.

# THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

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CONTENTS


| | | PAGE |
|---|---|---|
| *Report of Investigations into an Obscure Function of the Subconscious Mind* | HUGH WOODWORTH | 185 |
| *A Summer Session Course in Psychical Research at Harvard University* | GARDNER MURPHY | 231 |
| *Picture-Tests* | REV. W. S. IRVING | 256 |
| *Index to Volume XXXVI* (1942) | | 269 |


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2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.

3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.

4. Co-operating in the above tasks with qualified individuals and groups who will report their work to the Society.

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The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of great ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

## THE ENDOWMENT

The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psycho-therapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

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"I give, devise and bequeath to the American Society for Psychical Research, Inc., a corporation organized under the laws of the State of New York, the sum of ..... dollars (or if the bequest is real estate or other specific items of property, these should be sufficiently described for identification), in trust for the corporate purposes of said Society."

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